This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

THE FRANCES A. COUNTWAY LIBRARY OF MEDICINE
HARVARD MEDICAL LIBRARY-BOSTON MEDICAL LIBRARY

TRANSACTIONS

OF THE

ASSOCIATION OF AMERICAN PHYSICIANS.

THIRD SESSION,

HELD AT WASHINGTON, D. C.,

SEPTEMBER 18, 19, AND 20, 1888.

VOLUME III.

PHILADELPHIA:
PRINTED FOR THE ASSOCIATION.
1888.

DUPLICATE.

DORNAN, PRINTER,
N W. CORNER SEVENTH AND ARCH STREETS,
PHILADELPHIA.

CONTENTS.

PAGE

List of Officers for the year 1887–88 v
List of Officers for the year 1888–89 vi
List of Members vii
Constitution and By-laws xi
Minutes of the Third Annual Meeting xv

I. The President's Address. On the Relations of Scientific to Practical Medicine. By WM. H. DRAPER, M.D,, Prof. of Clinical Medicine in College of Physicians and Surgeons, New York 1
II. On the Geographical Distribution of Typhoid Fever in the United States. Its Relation to Fevers of Malarial Origin, and to Obscure and Unclassified Forms of Continued Fever. By W. W. JOHNSTON, M.D., Prof. of Theory and Practice of Medicine in the Columbia University, Washington, D. C. . 8
III. The Management of the Stage of Convalescence in Typhoid Fever. With Remarks as to its Sequelæ. By JAMES H. HUTCHINSON, M.D., Physician to the Pennsylvania Hospital, Senior Physician to the Children's Hospital, Philadelphia . 47
IV. Some Forms of Paralysis after Typhoid Fever. By GEO. ROSS, M.D., Prof. of Clinical Medicine, McGill University, Montreal 77
V. Fatty Overgrowth of the Heart. By F. FORCHHEIMER, M.D., Professor of Physiology and Clinical Diseases of Children in the Medical College of Ohio, Cincinnati 89
VI. Causal Therapeutics in the Infectious Diseases. A Clinical Experiment. By J. C. WILSON, M.D., Attending Physician to Phila. Hospital and to Jefferson Medical College Hospital 109
VII. The Diagnostic Significance of the Mitral Presystolic Murmur. By FRANK DONALDSON, M.D., Clinical Professor of Diseases of Chest and Throat, University of Maryland 121
VIII. On the Absolute and Relative Value of Albumin and Casts, and of Renal Inadequacy, in the Diagnosis of Diseases of the Kidney. By ROBERT T. EDES, M.D., of Washington, D. C., formerly Professor of Materia Medica, and Jackson Professor of Clinical Medicine in Harvard University . . 139
IX. Value of Albumin and Casts, and of Renal Inadequacy in the Diagnosis and Prognosis of Kidney Disease. By EDWARD G. JANEWAY, M.D., Professor of the Principles and Practice of Medicine in Bellevue Hospital Medical College, New York 163

PAGE

X. The Relation of Albuminuria to Life Assurance. By JAMES TYSON, M.D., Professor of General Pathology and Morbid Anatomy in the University of Pennsylvania, one of the Physicians to the Philadelphia Hospital, etc. . . . 171

XI. The Cardiac Changes in Chronic Bright's Disease. By ALFRED L. LOOMIS, M.D., LL.D., Professor of Pathology and Practice of Medicine in the University of the City of New York 180

XII. Chronic Interstitial Nephritis and Angina Pectoris. By SAMUEL C. CHEW, M.D., Professor of the Practice of Medicine, University of Maryland, Baltimore 207

XIII. The Treatment of Valvular Diseases of the Heart. By J. M. DA COSTA, M.D., LL.D., Professor of the Principles and Practice of Medicine in the Jefferson Medical College, Phila. 216

XIV. Disturbances of the Heart Rhythm with Reference to their Causation and their Value in Diagnosis. By G. BAUMGARTEN, M.D., of St. Louis 232

XV. The Pulse-wave Velocity and Ventricular Close-time in Health. By JAMES K. THACHER, M.D., Professor of Physiology and Clinical Medicine, in Yale University, New Haven, Conn. . 244

XVI. A Pin Imbedded in a Woman's Heart. By GEORGE L. PEABODY, M.D., Professor of Materia Medica and Therapeutics in the College of Physicians and Surgeons, New York . . 263

XVII. The Relation between Trophic Lesions and Diseases of the Nervous System. By E. C. SEGUIN, M.D., President of the American Neurological Association, etc. 266

XVIII. Effect of Spinal Concussion on the Reflexes. By F. T. MILES, M.D., Professor of Physiology and Clinical Prof. of Diseases of the Nervous System in University of Maryland, Baltimore 291

XIX. Contributions to the Anatomy and Pathology of the Thymus Gland. By A. JACOBI, M.D., Clinical Professor of the Diseases of Children in the College of Physicians and Surgeons, New York; President of New York Academy of Medicine . 297

XX. Recent Researches Relating to the Etiology of Yellow Fever. By GEO. M. STERNBERG, M.D., Major and Surgeon, U.S.A. 321

XXI. Pulsating Pleurisy. By WILLIAM OSLER, M.D., Professor of Clinical Medicine in the University of Pennsylvania . . 330

XXII. Antiseptic Medication. By FREDERICK P. HENRY, M.D., Physician to the Episcopal and Philadelphia Hospitals . 349

XXIII. Phthisis Pulmonalis. The Basis for its Therapeutics. By S. EDWIN SOLLY, M.D., Colorado Springs, Colorado . . 353

XXIV. Creasote as a Remedy in Phthisis Pulmonalis. By BEVERLEY ROBINSON, M.D., Professor of Clinical Medicine in Bellevue Hospital Medical College, New York 365

XXV. The Continued Fevers of the Mississippi Valley. By P. GERVAIS ROBINSON, M.D., of St. Louis 395

ASSOCIATION OF AMERICAN PHYSICIANS.

OFFICERS FOR THE YEAR 1887-88.

PRESIDENT.

WILLIAM H. DRAPER, M.D.

VICE-PRESIDENTS.

FRANCIS MINOT, M.D.

R. PALMER HOWARD, M.D.

RECORDER.

WILLIAM OSLER, M.D.

SECRETARY.

HENRY HUN, M.D.

TREASURER.

W. W. JOHNSTON, M.D.

COUNCIL.

ROBERT T. EDES, M.D.

HENRY M. LYMAN, M.D.

WILLIAM H. DRAPER, M.D.

FREDERICK C. SHATTUCK, M.D.

SAMUEL C. BUSEY, M.D.

WILLIAM H. WELCH, M.D.

WILLIAM PEPPER, M.D.

OFFICERS FOR THE YEAR 1888-89

PRESIDENT.

FRANCIS MINOT, M.D.

VICE-PRESIDENTS.

R. PALMER HOWARD, M.D.

SAMUEL C. BUSEY, M.D.

RECORDER.

I. MINIS HAYS, M.D.

SECRETARY.

HENRY HUN, M.D.

TREASURER.

W. W. JOHNSTON, M.D.

COUNCIL.

HENRY M. LYMAN, M.D.

WILLIAM H. DRAPER, M.D.

FREDERICK C. SHATTUCK, M.D.

SAMUEL C. BUSEY, M.D.

WILLIAM H. WELCH, M.D.

WILLIAM PEPPER, M.D.

GEORGE L. PEABODY, M.D.

MEMBERS.

* Original members.
† Deceased members.

*1886. Adams, J. F. A., M.D., Pittsfield, Mass.
*1886. Atkinson, I. E., M.D., 19 Cathedral St., Baltimore.
*1886. Ball, A. Brayton, M.D., 42 W. 36th St., New York.
*1886. Baumgarten, G., M.D., 2643 Chestnut St., St. Louis.
1888. Bridge, Norman, M.D., 550 W. Jackson St., Chicago.
*1886. Bruen, Edward T., M.D., 1531 Chestnut St., Philadelphia.
*1886. Busey, Samuel C., M.D., 1545 I St. (N. W.), Washington, D.C.
*1886. Carpenter, John T., M.D., Pottsville, Pa.
*1886. Chew, Samuel C., M.D., 215 W. Lanvale St., Baltimore.
*1886. Councilman, W. T., M.D., Baltimore.
*1886. Da Costa, J. M., M.D., 1700 Walnut St., Philadelphia.
1888. Dana, Charles L., M.D., 50 W. 46th St., New York.
*1886. Dana, Israel T., M.D., 181 State St., Portland, Me.
1888. Danforth, I. N., M.D., 294 W. Monroe St., Chicago.
*1886. Delafield, Francis, M.D., 12 W. 32d St., New York.
*1886. Donaldson, Frank, M.D., 510 Park Ave., Baltimore.
*1886. Draper, Frank W., M.D., 36 Worcester St., Boston.
*1886. Draper, William H., M.D., 19 E. 47th St., New York.
*1886. Edes, Robert T., M.D., 1216 18th St. (N. W.), Washington, D. C.
1888. Ernst, Harold C., M.D., Jamaica Plain, Mass.
*1886. Fitz, Reginald H., M.D., 18 Arlington St., Boston.
*1886. Folsom, Charles F., M.D., 15 Marlborough St., Boston.
*1886. Formad, Henry F., M.D., 3535 Locust St., Philadelphia.

*1886. FORCHHEIMER, F., M.D., 160 W. 9th St., Cincinnati.
*1886. GANNETT, WILLIAM W., M.D., 110 Boylston St., Boston.
*1886. GARLAND, GEORGE M., M.D., 227 Newbury St., Boston.
*1886. GRAHAM, J. E., M.D., Gerrard St., Toronto, Canada.
*1886. GUITÉRAS, JOHN, M.D., U.S. Marine-Hospital Service, Charleston, S. C.
*1886. HAYS, I. MINIS, M.D., 1004 Walnut St., Philadelphia.
*1886. HENRY, FREDERICK P., M.D., 721 Pine St., Philadelphia.
*1886. HOWARD, R. PALMER, M.D., 47 Union Ave., Montreal, Canada.
†1886. HUDSON, E. DARWIN, Jr., M.D., New York.
*1886. HUN, HENRY, M.D., 33 Elk St., Albany, N. Y.
*1886. HUTCHINSON, JAMES H., M.D., 133 S. 22d St., Philadelphia.
*1886. JACOBI, A., M.D., 110 W. 34th St., New York.
*1886. JANEWAY, E. G., M.D., 51 E. 25th St., New York.
*1886. JOHNSON, HOSMER A., M.D., 4 16th St., Chicago.
*1886. JOHNSTON, W. W., M.D., 1603 K St. (N.W.), Washington, D. C.
*1886. KINNICUTT, FRANCIS P., M.D., 42 W. 37th St., New York.
1888. LEWIS, MORRIS J., M.D., 1316 Locust St., Philadelphia.
*1886. LONGSTRETH, MORRIS, M.D., 1416 Spruce St., Philadelphia.
*1886. LOOMIS, ALFRED L., M.D., 19 W. 34th St., New York.
*1886. LUSK, WILLIAM T., M.D., 47 E. 34th St., New York.
*1886. LYMAN, HENRY M., M.D., 69 Dearborn St., Chicago.
†1886. MCBRIDE, THOMAS A., M.D., New York.
1887. MEIGS, ARTHUR V., M.D., 1322 Walnut St., Philadelphia.
*1886. MILES, F. T., M.D., 514 Cathedral St., Baltimore.
*1886. MINOT, FRANCIS, M.D., 65 Marlborough St., Boston.
*1886. MITCHELL, S. WEIR, M.D., 1524 Walnut St., Philadelphia.
1887. MUSSER, JOHN H., M.D., 3705 Powelton Ave., Philadelphia.
*1886. OSLER, WILLIAM, M.D., F.R.C.P. Lond., 1502 Walnut St., Philadelphia.
*1886. PEABODY, GEORGE L., M.D., 57 W. 38th St., New York.
*1886. PEPPER, WILLIAM, M.D., 1811 Spruce St., Philadelphia.
*1886. POLK, WILLIAM M., M.D., 7 East 36th St., New York.
*1886. PORCHER, F. PEYRE, M.D., 4 George St., Charleston, S. C.
1888. PRENTISS, D. W., M.D., Washington, D. C.

*1886. PRUDDEN, T. MITCHELL, M.D., 23 W. 33d St., New York.
*1886. PUTNAM, JAMES J., M.D., 63 Marlborough St., Boston.
1887. REEVES, JAMES E., M.D., 201 McCallie Ave., Chattanooga, Tenn.
*1886. ROBINSON, BEVERLEY, M.D., 37 W. 35th St., New York.
*1886. ROBINSON, P. G., M.D., 3411 Washington Ave., St. Louis.
†1886. ROCHESTER, THOMAS F., M.D., Buffalo, N. Y.
*1886. ROSS, GEORGE, M.D., 49 Union Ave., Montreal, Canada.
†1886. SCHMIDT, H. D., M.D., New Orleans, La.
*1886. SEGUIN, E. C., M.D., 419 Madison Ave., New York.
*1886. SHAKESPEARE, EDWARD O., M.D., 1336 Spruce St., Philadelphia.
*1886. SHATTUCK, FREDERICK C., M.D., 125 Marlborough St., Boston.
*1886. SHATTUCK, GEORGE B., M.D., 24 Marlborough St., Boston.
*1886. SINKLER, WHARTON, M.D., 1534 Pine St., Philadelphia.
*1886. SMITH, A. A., M.D., 40 W. 47th St., New York.
*1886. SOLLY, EDWIN S., M.R.C.S. Eng., 2 North Cascade Ave., Colorado Springs, Col.
1887. STARR, LOUIS, M.D., 1504 Walnut St., Philadelphia.
1887. STARR, M. ALLEN, M.D., 22 W. 48th St., New York.
*1886. STERNBERG, GEORGE M., M.D., U. S. A., Johns Hopkins University, Baltimore.
1887. STEWART, JAMES, M.D., 873 Dorchester St., Montreal, Canada.
*1886. THACHER, JAMES K., M.D., 76 Wall St., New Haven, Conn.
*1886. TRUDEAU, EDWARD L., M.D., Saranac Lake, N. Y.
*1886. TYSON, JAMES, M.D., 1506 Spruce St., Philadelphia.
*1886. WARD, SAMUEL B., M.D., 135 N. Pearl St., Albany, N. Y.
*1886. WELCH, WILLIAM H., M.D., 506 Cathedral St., Baltimore.
1887. WHITNEY, WILLIAM F., M.D., 228 Marlborough St., Boston.
*1886. WHITTAKER, JAMES T., M.D., 100 West 8th St., Cincinnati.
*1886. WHITTIER, EDWARD N., M.D., 285 Boylston St., Boston.
*1886. WILKINS, GEORGE, M.D., 898 Dorchester St., Montreal, Canada.
*1886. WILSON, JAMES C., M.D., 1437 Walnut St., Philadelphia.
*1886. WOOD, HORATIO C., M.D., 1925 Chestnut St., Philadelphia.

HONORARY MEMBERS.

1887. BILLINGS, JOHN S., M.D., U.S.A., Surgeon-General's Office, Washington, D. C.
1886. BOWDITCH, HENRY I., M.D., 113 Boylston St., Boston.
†1886. CLARK, ALONZO, M.D., New York.
1886. CLYMER, MEREDITH, M.D., New York.
1886. DALTON, JOHN C., M.D., New York.
1886. LEIDY, JOSEPH, M.D., 1302 Filbert St., Philadelphia.
1886. METCALFE, JOHN T., M.D., New York.
1886. STILLÉ, ALFRED, M.D., 3900 Spruce St., Philadelphia.

CONSTITUTION AND BY-LAWS.

ARTICLE I.

Objects of the Association and Name.

The Association has for its object the advancement of scientific and practical medicine. It shall be known as THE ASSOCIATION OF AMERICAN PHYSICIANS, and shall hold an annual meeting in the month of June in the city of Washington, D. C.

ARTICLE II.

Proceedings.

The Proceedings shall consist of:

1. Discussions on subjects of general interest in the departments of medicine and pathology. These subjects shall have been provided for by the appointment, at the previous meeting, of certain members, two upon each subject, to be called referee and co-referee, to open the debate on the subjects selected for discussion.
2. Original communications.
3. Demonstrations of gross and microscopic preparations, of apparatus, and instruments.

ARTICLE III.

Members.

There shall be Members and Honorary Members. The number of members shall be limited to one hundred. Physicians of sufficient eminence to merit the distinction may, to a number not exceeding twenty-five, be elected honorary members, and as such shall be entitled to attend all meetings and take part in the proceedings, but not to vote upon business questions.

Article IV.

Election of Members.

Nominations to membership shall be made on the first day of the meeting of the Association, and shall be referred to the Council, which shall report those nominations which are approved by it to the next annual meeting for action. A three-fourths vote shall be necessary for election of members, but for the election of honorary members a unanimous vote of those present shall be required.

Article V.

Annual Dues.

Each member shall pay an annual fee of ten dollars. Honorary members shall be exempt from fees.

Article VI.

Officers.

The officers shall consist of a President, two Vice-Presidents, a Secretary, a Recorder, a Treasurer—all to be elected annually, and a Council.

Article VII.

Duties of Officers.

The duties of the President, Vice-President, Secretary, and Treasurer shall be those usual to these officers. The Recorder shall secure the papers read and see that proper notes are taken of the discussions thereon, for the use of the Committee on Publication.

Article VIII.

Council.

The Council shall consist of seven members, who shall be chosen at the first meeting by ballot. One, selected by lot, shall retire at the end of the first year, and shall not be immediately eligible to re-election; another, similarly selected, shall retire at the end of each year, until all of those originally chosen shall be disposed of, after

which one shall retire each year, in the order of election, and his place be supplied by another.

Article IX.

Duties of Council.

It shall be the duty of the Council to suggest the subjects for debate, to consider the nominations for membership, and to report on them at the meeting at which they shall be balloted on. The Council shall also be a Committee on Nominations to Office, and as such shall present a report at the morning session of the last day, at the conclusion of which session the election shall be held by ballot.

It shall appoint a business committee of three, who shall, with the Secretary, make all arrangements for the meeting, including the preparation of a programme.

Article X.

Publication Committee.

The Secretary, Treasurer, and Recorder shall constitute a Committee on Publication, to which shall be referred all papers, reports, and other matters intended for publication.

Article XI.

Changes in Constitution and By-laws.

Proposals for changes in the Constitution and By-Laws must have been made at the meeting previous to that at which they are voted on, and the notices for which shall contain an announcement of the proposed changes; and such changes shall require, for their adoption, a majority vote of three-fourths of those present.

Article XII.

Expulsion of Members.

A member may be expelled from the Association for conduct unbecoming a physician and a gentleman. In such cases formal charges must be made by two members, which shall be referred to the Council

for recommendation. Membership shall lapse by reason of absence from three successive meetings without reason given acceptable to the Council.

Article XIII.

Quorum.

Any number of members present at the appointed time of the annual meeting shall constitute a quorum for the transaction of ordinary business; but for the election of members, fifteen shall be necessary for a quorum; and for the expulsion of members or for altering the Constitution and By-laws, twenty-five members shall be necessary.

Article XIV.

Order of Business.

1. The President shall call the meeting to order and open the session by an address not exceeding half an hour in length. In his absence the Vice-President shall preside, and in the absence of both of these officers the Chairman of the Council.

2. The debates shall be the next order of business, and shall be confined to the first morning session. The two members appointed to open the debate shall not occupy more than twenty minutes each. Subsequent speakers shall be restricted to ten minutes each.

3. Voluntary papers shall not exceed twenty minutes in the reading. In the discussion following the reading of such papers, remarks shall be limited to ten minutes.

4. Demonstrations shall be provided for in the afternoon sessions.

5. The referee and co-referee shall, whenever it is possible, send an abstract of their report to the Council, for distribution to members previous to the meeting.

6. In the morning session of the last day, of which it shall be the first business, the report of the Council as a Committee on Nominations to Office and to Membership shall be made, and ballot shall be held thereon.

MINUTES

OF THE

THIRD ANNUAL MEETING

OF THE

ASSOCIATION OF AMERICAN PHYSICIANS,

Held at Washington, D. C., September, 18, 19, *and* 20, 1888.

SEPTEMBER 18, 1888.—MORNING SESSION.

THE PRESIDENT, WILLIAM H. DRAPER, M.D., called the meeting to order at 10.30 A.M., in Hall No. 1, Grand Army Building, and delivered the Inaugural Address.

The following members were present:

From Philadelphia: Drs. J. M. Da Costa, Henry F. Formad, I. Minis Hays, James H. Hutchinson, Arthur V. Meigs, John H. Musser, William Osler, William Pepper, Edward O. Shakespeare, Wharton Sinkler, Louis Starr, James Tyson, James C. Wilson, Horatio C. Wood, and Edward T. Bruen.

From New York: Drs. Francis Delafield, William H. Draper, A. Jacobi, E. G. Janeway, Francis P. Kinnicutt, Alfred L. Loomis, William T. Lusk, George L. Peabody, William M. Polk, T. Mitchell Prudden, Beverley Robinson, E. C. Seguin, and M. Allen Starr.

From Boston: Drs. Reginald H. Fitz, Francis Minot, James J. Putnam, and Frederick C. Shattuck.

From Baltimore: Drs. I. E. Atkinson, Samuel C. Chew, W. T. Councilman, Frank Donaldson, F. T. Miles, and William H. Welch.

From Washington: Drs. Samuel C. Busey, Robert T. Edes, and W. W. Johnston.

From Montreal: Drs. George Ross, James Stewart, and George Wilkins.

From Cincinnati: Drs. F. Forchheimer, and James T. Whittaker.

From Chicago: Drs. Hosmer A. Johnson and Henry M. Lyman.
From Albany: Drs. Henry Hun and Samuel B. Ward.
From Pittsfield: Dr. J. F. A. Adams.
From St. Louis: Dr. G. Baumgarten.
From Portland: Dr. Israel T. Dana.
From Toronto: Dr. J. E. Graham.
From Charleston: Dr. F. Peyre Porcher.
From Chattanooga; Dr. James E. Reeves.
From New Haven: Dr. James K. Thacher.
From Colorado Springs: Dr. Edwin S. Solly.
From U. S. A.: Dr. George M. Sternberg.
The following foreign guests were also present:
Drs. David Ferrier, William M. Ord, P. H. Pye-Smith, A. E. Sansom, F. S. Eve, and Victor Horsley, of London.

On motion of Dr. James C. Wilson, the reading of the Minutes of the last meeting was omitted, and they were approved as published in the *Transactions*.

Dr. W. W. Johnston read his report as Treasurer, which was accepted, and Drs. Prudden and Welch were appointed an Auditing Committee.

Dr. William Osler, the Recorder, made an oral report.

On behalf of the Council, the Secretary read the following:

"The Council recommends, in view of the uncertainty in the estimated expenses for the coming year, that action on the proposed change of the Constitution, fixing the dues at twenty instead of ten dollars, be postponed till the next annual meeting."

This recommendation was adopted, and the consideration of the change in the Constitution was postponed till the next annual meeting.

Dr. W. W. Johnston then read a paper on the "Geographical Distribution of Typhoid Fever in the United States," which was discussed by Drs. Sternberg, Osler, Porcher, Wilson, Reeves, and Johnston.

Dr. James H. Hutchinson then read a paper on the "Management of the Stage of Convalescence in Typhoid Fever," which was discussed by Drs. Reeves, Peabody, Ord (London), Kinnicutt, Shattuck, Wilson, and Hutchinson.

Dr. George Ross then read a paper "On Some Forms of Paralysis after Typhoid Fever," the discussion of which was postponed until the afternoon session.

Adjourned.

AFTERNOON SESSION.

Meeting called to order at 3 P.M., by President Draper.

Dr. Ross's paper was discussed by Drs. Ord (London), Minot, M. Allen Starr, Osler, Wood, Delafield, and Ross.

Dr. Forchheimer then read a paper on "Fatty Heart," which was discussed by Drs. Jacobi, Kinnicutt, Shattuck, Tyson, Wood, and Forchheimer.

Dr. James C. Wilson read a paper on the "Causal Therapeutics in the Infectious Diseases," which was discussed by Drs. Sternberg, Dana, Peabody, Jacobi, Johnston, and Wilson.

Dr. Frank Donaldson then read a paper on the "Diagnostic Significance of the Presystolic Murmur," which was discussed by Drs. Thacher, Osler, and Donaldson.

The Secretary then read the nominations for membership.

Adjourned.

SEPTEMBER 19TH.—MORNING SESSION.

Meeting called to order at 10.15 A.M., by President Draper.

The discussion on the "Absolute and Relative Value of the Presence of Albumin and Casts, and of Renal Inadequacy, in the Diagnosis and Prognosis of Diseases of the Kidney," was opened by Referee Dr. Robert T. Edes, and Co-referee Dr. Edward G. Janeway; and Co-referee Dr. James Tyson read a paper on the "Relation of Albuminuria to Life Assurance," which was followed by a general discussion by Drs. Jacobi, Pye-Smith (London), Kinnicutt, and Porcher.

Dr. Loomis then read a paper on the "Cardiac Changes in Chronic Bright's Disease," which was discussed by Drs. Delafield, Peabody, Osler, Pye-Smith (London), and Loomis.

Dr. Chew then read a paper on the "Relation between Chronic Interstitial Nephritis and Angina Pectoris," which was discussed by Drs. Peabody, Minot, and Chew.

Adjourned.

AFTERNOON SESSION.

Meeting called to order at 2.35 P.M., by President Draper.

Dr. Da Costa read a paper on the "Treatment of Valvular Affec-

tions of the Heart," which was discussed by Drs. Shattuck, Chew, and Da Costa.

Dr. Baumgarten read a paper on "Disturbances of the Heart Rhythm, with Reference to their Causation and their Value in Diagnosis," which was discussed by Drs. Bruen, Edes, Osler, Lyman, Sansom (London), Draper, and Baumgarten.

Dr. Thacher then read a paper on "Pulse-wave Velocity and Ventricular Close-time in Health," which was discussed by Dr. Baumgarten.

Adjourned.

SEPTEMBER 20TH.—MORNING SESSION.

Meeting called to order at 10.10 A.M., by President Draper.

The following officers, nominated by the Council, were unanimously elected:

President.—Dr. Francis Minot.

Vice-Presidents.—Drs. R. Palmer Howard and S. C. Busey.

Recorder.—Dr. I. Minis Hays.

Secretary.—Dr. Henry Hun.

Treasurer.—Dr. W. W. Johnston.

Council.—Dr. George L. Peabody.

The following gentlemen, recommended by the Council, were elected members: Dr. Morris J. Lewis, of Philadelphia; Dr. D. W. Prentiss, of Washington; Drs. Norman Bridge and J. N. Danforth, of Chicago; Dr. C. L. Dana, of New York; and Dr. Harold C. Ernst, of Boston

Dr. Peabody then read a paper on "A Pin Found Imbedded in a Woman's Heart," with a demonstration of the specimen.

At 10.30, in conjoint session with the American Physiological Society, the Referee, Dr. Seguin, commenced the discussion on the "Relation between Trophic Lesions and Diseases of the Nervous System," which was continued by the Co-referee, Dr. Wood, and which was followed by a general discussion by Drs. Ord (London), M. Allen Starr, H. P. Bowditch, Pye-Smith (England), Ferrier (London), Horsley (London), Seguin, and Wood.

Dr. Miles then read a paper on the "Effect of Concussion of the Spine on the Reflexes," which was discussed by Drs. Edes, Osler, Wood, M. Allen Starr, and Miles.

Adjourned.

Afternoon Session.

Meeting called to order at 2.35 P.M., by President Draper.

Dr. Welch demonstrated a series of microscopical specimens of the thyroid gland after partial extirpation, and Dr. Prudden exhibited microscopical specimens of a thyroid gland from a case of myxœdema.

Dr. Shakespeare exhibited a number of photographs of bacteria.

Dr. Jacobi read a paper on the "Pathology of the Thymus Gland," illustrated by specimens and plates.

Mr. Frederic S. Eve (London) exhibited specimens of the microbe of syphilis.

Dr. Councilman demonstrated specimens, both macroscopic and microscopic, of "Bovine Tuberculosis."

Dr. Sternberg then read a paper on "Recent Researches Relating to the Etiology of Yellow Fever."

Dr. Dana moved that the Council be recommended to appoint the time of the next annual meeting in the latter part of September, 1889. Carried.

Dr. Pepper gave notice that at the next meeting of the Association he would offer an amendment to the Constitution requiring the Association's Representative on the Executive Committee of the Congress of American Physicians and Surgeons to consult with the Council of this Association before voting on the admission of any society to the Congress.

The following papers were read by title:

"On Pulsating Pleural Effusion," by Dr. William Osler.

"Antiseptic Medication," by Dr. Frederick P. Henry.

"The Therapeutics of Phthisis," by Dr. Edwin S. Solly.

"Creasote as a Remedy in Pulmonary Phthisis," by Dr. Beverley Robinson.

"Continued Fevers of the Mississippi Valley," by Dr. P. Gervais Robinson.

Adjourned.

Henry Hun, *Secretary.*

NOTICE.

THIS volume of TRANSACTIONS contains the papers read before the Association at the meeting held September 18, 19, and 20, 1888.

The Association assumes no responsibility for the statements and opinions published in this volume.

THE PRESIDENT'S ADDRESS.

ON THE RELATIONS OF SCIENTIFIC TO PRACTICAL MEDICINE.

By WILLIAM H. DRAPER, M.D.,
PROFESSOR OF CLINICAL MEDICINE IN THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

I FEEL that I should best acknowledge my appreciation of the object of this Association, and my sense of the honor of presiding over its deliberations, if I were allowed to proceed directly to the work we have in hand, and to conduct, as I may be able, the important proceedings of this meeting.

We are met as "friends in council," to present and compare the results of our individual experience, to discuss the questions which vex us, and to add something, if possible, to the enlargement of the science and the improvement of the art which constitute the aim and purpose of our lives.

The Council has decreed, however, that there must be a formal introduction to this function, and, instead of following the excellent example of my predecessors, and showing my respect for your precious time, I am compelled to subject you to an infliction from which I would gladly stay my hand.

I will promise, for your consolation, that my exordium shall be brief, and I must ask, in return, what I shall greatly need, your kind indulgence for my shortcomings in the task which is before me.

The avowed object of our Association is the advancement of scientific and practical medicine, and it has seemed to me appropriate to this occasion to submit to your consideration some reflections upon the relations which scientific and practical medicine bear to each other.

We may differentiate the science from the art of medicine, but we cannot practically dissociate them. In their ideal union they are

interwoven like warp and woof, built into each other like foundation and superstructure. Scientific medicine, to be sure, is not always practical, but it is ever striving to become so, and practical medicine, though not always, in a strict sense, scientific, is constantly tending to that end. And yet the distinction between science and art in medicine is as important to maintain for the secure progress of both as it is in any other department of human activity. The scientist pursues science for the sake, primarily, of knowing what is veiled in obscurity; the artsman cultivates science for the sake of learning how to do something which needs to be done. In medicine, perhaps, less than in any other occupation where science and art are combined, has this distinction been sharply drawn, for the reason I have stated, that scientific medicine is always seeking to become practical, and practical medicine to become scientific. More than this, the progress of science in medicine is largely dependent for its inspiration on the demands which the art is constantly making upon it for the solution of its problems, and the art constantly thrives by testing and correcting the theorems which science furnishes for the forming of its rules.

In spite, however, of the closeness and importance of this mutual relation, it is necessary to remark that the worker in the science and the worker in the art of medicine occupy essentially distinct positions, and where they are combined, as they are often in a high degree in the same individual, the aim and the methods peculiar to each must be constantly kept in view. The one is a naturalist, the other is an artsman; the one is engaged in searching out the ways of nature in disease, the other in using the knowledge thus attained in doing something for its detection, its prevention, or its cure. The pathologist is interested in investigating disease uninfluenced by the disturbing action of remedies; the practitioner is chiefly occupied with pathology as formulating the problems which devolve upon him to solve. The pathologist is concerned chiefly with the causes and effects of disease, the practitioner is absorbed in its dynamics. It is here especially that the practical man necessarily becomes in a high degree a scientist, for it is here that he occupies a field of observation which is peculiarly his own.

The pedigree of disease, the variations in its type due to race and to personal idiosyncrasies, the varieties and correlations of the structural and functional derangements which flow from one and the same con-

stitutional vice, the protean reactions to food and medicine, and the relations of all these peculiarities to heredity, to age, to sex, to habits, occupations, and environment; these constitute the special province of the scientific work of the practical physician, and require essentially the scientific spirit for their successful cultivation.

It is manifest, therefore, that while scientific and practical medicine are both working toward a common end, the proper function of each is yet distinct and separate.

The tendency at the present day is undoubtedly to accentuate this distinction and to separate the investigation of the causes, effects, and variations of disease processes from the art which is concerned with their diagnosis and differentiation as well as with their prevention and cure; and yet for the reason we have just suggested this distinction cannot be pushed too widely, and even in a scientific point of view the intelligent practitioner will always be at a certain advantage as compared with the merely scientific man. The multiplication of laboratories and of workers in technical schools of physiology, chemistry, and pathology is constantly increasing; and, on the other hand, the enlargement and systematic development of the opportunities for clinical observation and experience in hospitals are progressing, not, perhaps, with equal step, but with a growing sense of their essential and independent importance. It is hardly necessary for me to point out the mutual advantages to scientific and practical medicine which have resulted from this specializing of their separate functions.

The observations of the physician have not only suggested to the scientist some of the most interesting problems in biology but some of the most important factors in their solution. The history of pathology is full of illustrations, in medicine and surgery, of additions to our knowledge of diseases which would never have had anatomy or name but for the experience of practical physicians. The pages of physiology are illuminated with brilliant discoveries which had their origin or their confirmation in the symptomatology of disease; and, on the other hand, all the advances in these sciences, and in every science which is tributary to our knowledge of disease, have modified in many ways the methods, and helped the progress of the healing art. But if I need not enlarge upon the advantages to scientific and practical medicine from the specializing of their separate functions, I may be

permitted to draw attention to some of the disadvantages which are perhaps inseparable from their independent cultivation.

I have said that the tendency of science is always to become practical, and it must necessarily sometimes draw premature conclusions from its premises. The theorems of science are often incomplete, seldom, indeed, sufficient to meet at once all the requirements of art. The only competent critic of their practical value, the only trustworthy judge of the precepts which they suggest, is the practitioner who tests them.

It has been truly said that knowledge, like our food, must be digested before we can convert it into useful energy. The facts of science are of no more use as materials for practical work than the ultimate elements of our food would be for nutrition; they must first be transformed into new combinations, and then pass through a series of more or less complex changes before they can be made to yield up the force which makes them a power for good and useful achievements. The many beneficent applications of electricity, for example, have not been made by men who were eminent as physicians, but by men who were endowed with a sort of divine instinct for adapting the laws which govern this subtle force to the practical needs of humanity.

From its beginning the medical art has been struggling to rid itself of superstitions and delusions. It has always been hampered by the tyranny of tradition, and in our day it is bewildered by the revelations of science. How to assimilate the discoveries in the etiology of disease, in physiology, in organic chemistry, and in psychology, and how to make them all contribute to more exact diagnosis and more rational therapeutics, are the great problems which ever lie in the path and confront the conscience and the resources of the practical physician. As I have said, there is always the danger that science may become prematurely practical, and art, therefore, falsely scientific. It is well to remember that in one sense practical is always in advance of scientific medicine, and that in another it is always in its train. It is always in advance because it is ever referring new problems to science to solve; it is in its train, because it has continually to test and digest the theorems which science suggests for their solution.

There is no better illustration of this statement than that which is afforded by the history of parasitism, the latest and most portentous contribution which science has made to the theory and practice of the

healing art. The observations and speculations of practical physicians may be said to have led up to this discovery, and the revelations of scientific investigation have suggested the theorems which are now being tested by the practical physician. Already the applications of these theorems may be said to have reformed surgery, and to have given a new and hopeful direction to the rational treatment of disease, but it is not yet safe to affirm that the doctrine of parasitism may not be forging new errors in diagnosis, and beguiling us into dangerous paths in therapeutics.

Bacteriology indeed affords a curious and interesting example how, in the cycle of human knowledge, history repeats itself, and how ancient ideas present themselves clothed in new forms, but revealing the same essential nature. For what is the germ theory of disease but a concrete form of the abstract idea that disease is an entity, an enemy that invades and destroys, that must be contended with as a foe, and exorcised as a demon? It is the old "deus ex machina" come back to us to explain the mystery of disease. This time he is, to be sure, tangible; we can distinguish the variety of his hosts, and count his battalions. He is no longer the "pestilence that walketh in darkness," and we are entering upon the crusade against him as if he were the cause of all our woes. He is verily become a germ of universal discord.

But not only has the germ theory revived, in more exact and tangible form, the ancient pathology; it has reinaugurated, as a natural and necessary sequence, experimental efforts to destroy the germ; and as, in the olden time, the attempts to exorcise the demon often led to the destruction of the victim, so, in these latter days, there is danger lest the war upon parasites should add new and worse perils to their hosts. It is still possible that we may not only exaggerate the influence of germs in the production of disease, but the necessity also of destroying them, at all hazards, as the only principle involved in antagonizing their effects. We may well believe that we are on the eve of momentous changes in the healing art, resulting from discoveries in bacteriology, but these changes must come mainly from the independent and judicious application of these new ideas to the study and treatment of disease by the practical physician at the bedside.

Already the relations of bacteria to the pathology and treatment of fever furnish an illustration of the way in which clinical observation

enlarges and complicates the problem which the pathologist has to solve. Until we know how microörganisms produce fever, whether by their irritation of nervous centres, or by the increased oxidation incident to their multiplication, or by the development of ptomaines, or in some other as yet unexplained way, until we know whether the fever they produce, however produced, is a salutary or a dangerous process, to be antagonized or let alone, we are really in no better position to treat fever rationally than we were before the etiological relation of bacteria to fever was suspected. Were it not that clinical observation and experience are thus continually complicating the problem and opposing new obstacles to the precepts of experimental pathology, a check would not so soon have been given to the indiscriminate use of antipyretics and parasiticides.

I have cited bacteriology as one illustration, out of many which might be given, of the independent nature and special aim of scientific medicine, and to show how practical medicine must pursue, in dealing with scientific theorems, an equally independent method in testing and applying the precepts which these theorems suggest. And as scientific medicine has divided and specialized its work, in the many departments of science which contribute to our knowledge, so practical medicine, by the specialization of its aims and the division of its labor, illustrates the independent growth and progress of the medical art.

In the Congress which has gathered here at this time we have a significant acknowledgment of this fact, and an abundant confirmation, if any were needed, not simply of the mutual dependence of the science and the art of medicine, but of their independent developments, science ever seeking to solve the problems in the path of art, and art determining for itself its own limits and possibilities.

We have said that there is a tendency in scientific medicine to become prematurely practical, and that in practical medicine the tendency is sometimes to become falsely scientific; the only way to avoid these perils seems to be by maintaining, where it is possible, clear ideas as to the mutual relation upon which I have insisted, and to encourage the independent cultivation of the science and the art. The fault of medical education in this country is the failure to indoctrinate the principles and demonstrate the practice of clinical medicine along with the purely scientific knowledge which is the essential preparation for it. It is true that in our larger metropolitan schools in-

creasing attention is being paid to clinical instruction, but it is only to a limited degree compulsory, and the result is that a considerable proportion of the graduates in medicine are forced to begin their careers as practitioners with a measure of the necessary science but without the rudiments of the required art. Too many of them are such physicians as Montaigne describes when he likens them to "him who paints the seas, rocks, and heavens, and draws the model of a ship, as he sits safe at his table; but send him to sea and he knows not how or where to steer; so doctors oftentimes make such a description of our maladies, as a town crier does of a lost dog or donkey, of such a color and height, such ears, etc., but bring the very animal before him and he knows it not for all that."

But not only are these new-fledged doctors launched upon the serious responsibilities of their profession without the experience which would enable them to recognize and differentiate diseases; they are often without intelligence in the practical use of the means of contending with them. They are in danger, therefore, of becoming sceptics in therapeutics, or, what is worse, hobbyists. Many of them drift into specialism, which in its largest and best sense can only be exercised by those whose expert knowledge and skill are based upon the broad and secure foundation of general pathology, and they too often degrade what is truly an art to the level of a craft.

In offering these reflections upon the relations of scientific and practical medicine I am only too conscious that they will seem quite unnecessary to be spoken in the presence of such an audience as this; but if any extenuation is needed for their utterance it may be found, I think, in the fact that we are most of us engaged more or less actively as teachers either of the science or the art of medicine, and it is impossible to exaggerate the responsibilities which these functions devolve upon us. We must pursue science so as not to imperil the progress of art, and we must cultivate art as if science were not so much its mistress as its handmaid. We may wisely recall and apply the answer which Iphicrates the Athenian General is said to have made when he was hard pressed by an orator before the people, to say what he was to be so proud: "Are you a soldier, a captain, an engineer, a spy, a pioneer, a sapper, or a miner?" "No," said Iphicrates, "I am none of these, but I command them all." So the practical physician may not be a very learned pathologist, an erudite physiologist, a skilful

chemist, or an expert bacteriologist, but he must command them all, and it is he who must finally, through the coöperation of all the sciences which they represent, perfect the most beneficent of all arts; it is he who in his high mission as the servant of humanity must attain that wisdom which results from combining knowledge with the instinct and the skill for its useful application.

ON THE GEOGRAPHICAL DISTRIBUTION OF TYPHOID FEVER IN THE UNITED STATES.

Its Relation to Fevers of Malarial Origin, and to Obscure and Unclassified Forms of Continued Fever.

By W. W. JOHNSTON, M.D.,
PROFESSOR OF THEORY AND PRACTICE OF MEDICINE IN THE COLUMBIAN UNIVERSITY, WASHINGTON, D. C.

The purpose of the present paper is to aid the study and classification of typhoid fever and its modifications, as they are seen in the United States, by defining their geographical distribution, their resemblances, and their differences from other forms of continued fever. It is very apparent that this study and classification are by no means complete, nor even well advanced toward completion; that great differences in opinion prevail on many essential questions, as is shown in discussions in State and local societies; and that the observation of the same phenomena, constantly repeated, does not lead to the same conclusions.

As typhoid fever is the chief object of study in this paper, by comparison with all other forms our continued fevers may be divided into two classes: 1. Typhoid fever; 2. All other continued fevers. In the first class would be included those fevers only which have a sufficient number of the characteristic symptoms of typhoid fever to render the diagnosis reasonably certain. In the second class are all forms of continued fever which have no such aggregation of symptoms, or which have them in modified degree. It includes malarial continued fever and its varieties, typho-malarial fever, atypical typhoid fever, thermic fever, autumnal fever, simple continued fever, etc.; in other words, all forms of continued fever which are not usually recognized as typhoid fever.

There are many difficulties in the way of determining how prevalent

typical typhoid fever is, and its relative degree of prevalence in different portions of the country. These difficulties can be stated to be:

a. The lack of full and sufficiently accurate reports of the number of cases and deaths. (Approximate data are found in the General Census returns and in the Reports of Boards of Health, U. S. Army and Navy Reports, and in the Reports of the United States Marine-Hospital Service.)

b. The differences of opinion as to the nature of many of the forms of fever which are included in the second class; for while certain symptoms are by some held to indicate the existence of atypical forms of typhoid fever, many such cases are returned as continued malarial or typho-malarial, or other forms of fever. This statement is based upon the expressed opinions of physicians who assert that such contradictory views are held. Therefore, no matter on which side the error lies, and an error there must be, the correctness of all statistical conclusions will be untrustworthy, for the number of reported cases of typhoid fever will be in excess or fall short of what it should be. This indeterminate class of fevers constitutes a very large proportion of the prevailing fevers, and it makes a very great difference whether the statistics of typhoid fever receive or do not receive large accessions from it.

In order, however, to arrive at more precise notions of prevailing fevers, the study should go beyond the death-rates, and beyond limited hospital and board of health reports. A fatal termination to a continued fever case creates a certain bias of opinion at once as to its nature, and leads to the affixing of a graver title to it. It is not the fatal cases which are so difficult to classify, for there is the opportunity here for post-mortem rectification, but it is the non-fatal cases, the mild, doubtful forms, about which medical science is uncertain, and individual opinion divided. The number of deaths from typhoid fever does not by any means indicate its distribution, as the cases of enteric fever may be more numerous or less fatal where a mild type prevails, and more fatal and less numerous where the type is severe. All that we can learn from ascertaining the number of deaths from enteric fever is that the death-rate is higher in some localities than in others. There is, therefore, no way to reach any result here, except by knowing what individual opinions are, and upon what facts these opinions are based.

Hence, the following list of questions was prepared and distributed to physicians; the larger number being sent to malarial regions:

1. To what extent does typhoid fever prevail in your locality and in your practice?
2. Is it more prevalent in your practice now than formerly?
3. Is the prevailing type severe, of medium severity, or mild?
4. Are the cases which you recognize and call typhoid fever, as a rule, well defined, having most of the typical symptoms of this disease?
5. Are the following symptoms usually present or not?
   (*a*) Rose spots.
   (*b*) Tympanites.
   (*c*) Diarrhœa.
   (*d*) Intestinal hemorrhage.
   (*e*) Enlarged spleen.
   (*f*) Bronchial catarrh.
   (*g*) Delirium.
6. What is the average duration?
7. Are relapses common?
8. What is the percentage of fatal cases?
   What is the usual cause of death?
9. Do malarial fevers prevail to any great extent in your practice, and are they more or less prevalent than formerly?
10. Do you recognize a continued malarial (bilious or remittent) fever which can be differentiated from typhoid fever?
11. Do you think that the typhoid fever which you see is modified by malarial infection?
12. Do you recognize in your practice a distinct type of continued fever which is neither malarial nor typhoid, or one which is a compound of both—a typho-malarial fever—and what are the characteristic symptoms and lesions?

From the three hundred and fifty answers returned to these questions, a very fair conclusion can be formed as to prevailing opinions on this subject, and as this paper is an effort to review and classify these opinions, its title might very well be given as: "A consideration of the geographical distribution of ideas on the subject of the continued fevers of the United States."

All continued fevers were divided into two classes for comparison, the second class having as its type malarial fever, and, as the greatest difficulty lies in separating typhoid fever in its less characteristic forms from malarial fever of continued type, the geographical distribution of malarial fever will afford the best basis for the study of these differ-

ences. Where there is less malarial fever such differentiation is easier, where there is more malarial fever the difficulties augment.

I have in this present paper found it necessary to confine my attention to a limited portion of the country (deferring to another time the study of the rest), and have selected certain areas where enteric fever is found with none, or with very little paludal disease, and other areas where malaria and enteric fever occur side by side, and where they can be contrasted. The maps of malarial and typhoid fever distribution of the census of 1880, when placed side by side, will show the distribution of mortality from these two causes, and their relations to each other. (Maps exhibited.)

Three regions distinguished by differences in the mortality from malarial diseases (the difference being indicated in the Census Maps by different colors) are selected for discussion in this paper; these regions are:

I. NON-MALARIAL REGION.—Maine, New Hampshire, Vermont, Massachusetts, New York (except southeast corner), Pennsylvania, West Virginia, Virginia (West), North Carolina (West).

II. SUB-MALARIAL REGION.—Connecticut, Southeastern New York and Long Island, New Jersey, Delaware, Maryland, and the District of Columbia, Virginia (East), North Carolina (Centre).

III. MALARIAL REGION.—North Carolina (East), South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Texas, Arkansas, Kansas, Missouri (South), Kentucky (West), Tennessee (West).

REGION I.—NON-MALARIAL.

CLASS I.—Enteric fever of a typical character is said to prevail to a limited extent in this area. Seven correspondents only say that it is very prevalent; the majority think that it is not more frequent now than formerly; and five dissent from this statement.

The disease recognized as typhoid fever in these answers is, according to the greater number, well defined and typical; that is, when the disease is said to have a limited prevalence the typical form of enteric fever is meant.

To questions asked with a view of determining the relative frequency of the more typical symptoms, the following answers were made: Opinion is equally divided as to the usual presence and absence

of rose rash. By the majority tympanites, diarrhœa, bronchial catarrh, and delirium were said to be the usual and more common symptoms, while enlarged spleen was said to be rare. It was the unanimous statement that intestinal hemorrhage was very rarely or never found. Such unanimity of assertion is not found in regard to any symptom in any other region considered in this paper.

The prevailing type is said to be of mild or medium intensity. The duration is given at four weeks. Only one places it at two weeks, eight at three weeks, and four at five weeks. All, with a few exceptions, state that relapses are common. The percentage of deaths ranges from two to twenty. One-third of the answers give it as ten per cent. The most common causes of death are exhaustion of the heart and long-continued pyrexia; death from perforation and hemorrhage is rare.

The enteric fever of this locality may be said, therefore, to be a fever of about four weeks' duration, rarely fatal, with many of the typical symptoms—rose spots, intestinal hemorrhage, and enlarged spleen being the least common.

CLASS II.—The continued fevers in this territory which are not recognized as typhoid fever, are very generally disseminated and form the bulk of the febrile cases. They are the anomalous, doubtful, atypical cases which await designation and classification. They receive various names as bilious or remittent fever, autumnal, simple continued or gastric fever.

As to the fevers of paludal origin, these reports say that they are very rarely seen except as an imported disease; in the greater part of this area they are said not to occur at all. The absence of malarial influence in the causation of disease has a very important bearing on the study and classification of the continued fevers, which are not clearly typhoid. The determination of their etiology, pathology, and diagnosis is thereby rendered an easier matter. Such anomalous cases, too, become a standard for comparison for other anomalous fevers prevailing in undoubted malarial localities.

While denying that malarial disease exists, yet it is stated that physicians in such localities speak of a "bilious fever," meaning, necessarily, a fever without malarial infection. "We have no malarial disease unless imported, but have a low form of bilious fever that I do not recognize as typhoid." "I think I can recognize a 'bilious re-

mittent fever' which can be differentiated from typhoid fever, but I do not think we can trace it to a specific malarial influence."[1]

"The 'bilious remittent fever' can be distinguished by the absence of rose spots and tympanites and diarrhœa; it is always attended by constipation."[2] "Its symptoms seem, therefore, to be of a negative character; a continued fever with none of the characteristic symptoms of typhoid fever. More or less hemorrhage from the bowels may occur in these cases."[3]

In Parkersburg, W. Va., "all the forms of fever seem to be more or less complicated with malaria, so much so that it is almost impossible to treat it successfully without the use of quinine in considerable quantities."[4]

As to the nature of this so-called bilious fever, there is some difference of opinion. The term "bilious" is not used by some to imply malarial features in its symptoms, but to qualify the disease with the idea of obscure nature; by others, it hints at a malarial origin. Speaking of this class of cases, one correspondent says that "many of our cases are said to start with a train of symptoms for ten days or two weeks, and then to change to typhoid. The only continued fever that we get in this locality is of a typhoid type, ranging from a severe to a very mild form. We never have any mixed continued fevers. The cases are all typhoid from the start."[5]

"I have for a long time believed them (bilious or remittent fever and typhoid fever) to be one and the same disease."[6]

Autumnal fever "is a name given to a fever occurring in the fall months; it has no symptom in common with enteric fever, except morning and evening slight rise and fall in temperature—the temperature does not exceed 102°; it is usually of a mild type; recovery is the rule."

Simple or common continued fever is the name frequently given to these cases; its symptoms are "loss of appetite, general weakness, constipation, occasional chills, irregular pains in the head and back, continuing from two weeks to as many months."[7] "Great prostration, absence of delirium, rose spots and tympanites, very slight rise of temperature, but very rapid pulse, often as much as 140."[8]

[1] Walpole, N. H. [Where names of physicians are omitted, they failed to append their signatures.]
[2] Clarkesburg, W. Va.
[3] Bath, Maine, Dr. A. J. Fuller.
[4] Parkersburg, W. Va., Dr. L. C. Hunt.
[5] Brandon, Vermont, Dr. C. W. Peck.
[6] Brandon, Vermont.
[7] Portland, Maine, Dr. Thos. A. Foster.
[8] Clarkesburg, W. Va.

Gastric fever is the designation of a continued fever in this region. The symptoms are said to be "sensation of fulness in the stomach, flatulency, nausea, vomiting, tongue thickly furred, bowels costive. I treated one case in which typhoid fever symptoms developed three weeks after the first attack (of gastric fever). The patient, forty-three years old, robust, became emaciated and died five weeks after the commencement of the disease."[1]

"We sometimes have a continued fever without marked typhoid characteristic symptoms, called by some bilious, by others gastric, by others simple continued fever. These cases, as a rule, are of shorter duration than a well-defined case of typhoid. In them abdominal symptoms are absent."[2]

"In this section we have a simple (gastric? mountain?) fever, in which about the only symptom is fever, and which runs its course in about ten to twelve days."[3]

Atypical Typhoid Fever.—Many practitioners throughout this territory recognize fevers of slight intensity, without definite or peculiar symptoms, as being atypical typhoid fever. "There have been quite a good many cases of mild character," says one correspondent, "lacking the typical symptoms of typhoid, but I think significant enough to make me believe them due to the same poison that caused typical ones—simply a difference in degree as we find in scarlatina, measles, variola, etc."[4]

"I recognize the existence of a 'continued fever' which, to me, seems in many cases, but not all, to be a modified typhoid—the difference being in degree, and the etiology being a common one."[5]

"I was called to see several of the cases. The physicians in attendance were at a loss to classify or diagnose the cases. I diagnosed them typhoid fever, yet the cases that I saw were not well defined, not having the typical symptoms well marked. I saw no case that I could call a typical case of typhoid fever."[6]

"We frequently have here a mild form of fever, called 'spurious typhoid,' in which there is no diarrhœa, the bowels being often constipated, but little delirium, the temperature rarely going above 103°.

[1] Dover, N. H.
[2] Dover, N. H.
[3] Saranac Lake, Dr. E. L. Trudeau
[4] Portland, Maine, Dr. S. C. Gordon.
[5] Portsmouth, N. H., Dr. John J. Berry.
[6] Fairmont, W. Va.

I think it almost certain that this is a genuine typhoid, but mild in form; it is much more common now than formerly, and has apparently taken the place of the more severe type."[1]

Region II.—Sub-malarial.

This region includes Connecticut, Southeastern New York, Long Island, New Jersey (the city of Philadelphia is at its edge), Delaware, Maryland, the District of Columbia, and Eastern Virginia. It has a malarial ratio of 30 to 49 per 1000 deaths, and Region I. (non-malarial) has a ratio of under 10 per 1000; and Region III. (malarial) a ratio of 50 to 70 and over per 1000 deaths. In some portions of this region there is more paludal disease than elsewhere, as in Eastern Maryland and Eastern Virginia, and as compared with the large cities of New York, Philadelphia, Baltimore, and Washington, this difference of prevalence is very great.

I have addressed but few requests for information as to predominant opinions in this region, because the majority of the members of this Society are from this region, and have either contributed papers or expressed their views as to the types of fever prevailing here, and because material for study is more easy of access.

Class I.—In this region typhoid fever may, without dispute, be said to have a very general distribution. It is the continued fever of cities, and it has a very great rural prevalence as well. It rarely becomes extensively epidemic. That it has increased, and that this increase has been most marked in cities where malarial fevers have undergone the greatest diminution, there can be no doubt. This increase has been chiefly of cases of the so-called atypical, or mild cases of typhoid, and in the paper of Dr. Atkinson[2] read before this Society last year, and in the contributions of other members to medical journals, there is seen the evidence of a growing belief in the prevalence of a form of typhoid fever, which is like remittent fever in its sudden mode of onset with chills, and which runs a course of three to four weeks, the temperature conforming more or less to the typical type, but with no cerebral or intestinal symptoms, with no rose spots, or with these symptoms present to a slight degree. While this is true

[1] Charlottesville, Va., Dr. William C. Dabney.
[2] Trans. Assoc. Amer. Physicians, p. 209, 1887.

of the cities and less malarial parts, it is equally true of the rural districts and the more malarial parts of this region.

The reports which I have received from Eastern and Southeastern Virginia, show that typhoid fever is increasing in prevalence, as "in the country near Norfolk, where it now prevails to a greater extent than the old bilious remittent fever."[1] The cases called typhoid here are only those which present typical symptoms more or less numerous, and yet rose spots are rarely seen; tympanites, bronchial catarrh, and delirium, are usually met with; diarrhœa may be often absent; intestinal hemorrhage and enlarged spleen are occasionally seen. The attack lasts from three to six weeks. Relapses are not common and the fatality varies from eight to twenty-five per cent. Death has been due to cardiac exhaustion; rarely from perforation or intestinal hemorrhage.

CLASS II. *Malarial Fevers, Atypical, Anomalous Fevers, etc.*— The lessening of fatality from malarial disease in the cities of this region is proved by statistics. What the views are as to the prevalence of continued malarial or bilious remittent fevers may be judged by the views expressed by the President of this Association last year, by Atkinson in his excellent paper, and by Jacobi. Dr. Draper, speaking from his own experience and that of others, said that "cases of remittent fever which could easily be confounded with typhoid fever are not common. The cases of remittent fever which are seen in New York are chiefly imported from Southern ports."[2] Dr. Atkinson says that "remittent malarial fever is so simulated by the cases under discussion that the diagnosis must wait upon the crucial test of treatment. From beginning to end they lack the most characteristic symptoms of typhoid fever, and should be diagnosticated from the persistence of fever, its defiance of specific treatment, and their occasional dependence upon the typhoid principle."[3]

If the opinions of others were to be a guide, I should say that a continued fever, due solely to malarial infection, is met with in the cities of this region as an unusual form of disease. As far as my own observation goes, I am entirely in accord with Dr. Draper, that practically there is no such disease as remittent or continued malarial fever here. There is nothing in the history of these cases or in the result

[1] Norfolk, Va., Dr. Herbert M. Nash.
[2] Trans. Assoc. Amer. Physicians, p. 220, 1887.
[3] Op. cit., pp. 219, 220.

of therapeutic tests to prove their malarial origin, and much in their history and complications to prove that they are all cases of enteric fever.

Region III.—Malarial.

This region comprehends the area in which deaths from malarial diseases occur in the ratio of 50 to 70 and over per 1000 deaths, and comprises the States of the South Atlantic Coast, the Gulf Coast, and Southwestern States.

If malarial prevalence is compared with the distribution of enteric fever in this region, it will be seen that there is a great difference; but in certain parts, as in Central North Carolina, Northern Georgia and Alabama, Eastern Texas, except the coast line, Southern Missouri and Western Kansas, typhoid fever reaches its highest ratio—from 45 to 65 and over to 1000 deaths. So that we may say that in this region malarial fever reaches the highest average, and in a considerable portion of the region enteric fever also reaches its highest average. Outside of this region, in a strip of Western Iowa and on the coast of Oregon only, is enteric fever as prevalent. It may be said, therefore, that this is the region in which the two diseases prevail side by side in their greatest intensity, that here is the debatable ground, where doubt will be most justified in cases not typical, and where certainty can only come through the most careful study and observation of the facts.

For convenience of study and classification I have subdivided this large area into three groups:

First group: North Carolina (East), South Carolina, Georgia, and Florida.

Second group: Alabama, Mississippi, Lower Louisiana, and the Gulf Coast of Texas.

Third group: Greater portion of Texas, Upper Louisiana, Arkansas, Kansas, Lower Missouri, Kentucky, and Tennessee.

First Group.

Class I.—In this group typhoid fever is said, by the larger number of correspondents, to have a limited prevalence, in a form of medium and mild intensity. Those who say that it is more frequent than

formerly, and those who say that it is less so, are equally divided in number. Only eight replies state that the disease appears in a severe form.

It is noted that those who speak of the disease as being very rare, assert that the few cases which they have seen were of a severe type, but all, with the exception of twelve, say that the disease has well-defined characters, so that the diagnosis is not difficult. In other words, all refer, when speaking of typhoid fever, to cases more or less conforming to the type. So that statements as to prevalence, fatality, and frequency of symptoms refer to such typical cases only.

Two-thirds reply that rose spots are infrequent; one-third, that they are frequent. Tympanites, diarrhœa, and bronchial catarrh are the more common symptoms; delirium is as often absent as present; intestinal hemorrhage is rare; enlarged spleen nearly as much so. The average duration is from three to four weeks, it may last five to ten weeks. Relapses are common, only four saying to the contrary. The percentage of fatal cases is given by the majority as 10; it ranges, however, from 5 to 50 per cent. Exhaustion is the most frequent cause of death; perforation and intestinal hemorrhages have the same place in frequency. With the blacks it is more fatal than with the whites.

CLASS II.—Malaria, as judged by its mortality, has a great prevalence in this group. In the rural portions of the South Atlantic region there are 70.6 and in the cities 11.5 deaths in 1000 from this cause. A portion of this group—parts of North Carolina, South Carolina, and Georgia, belonging to the southern hills and plateaus—has a percentage of 69.3 in 1000 deaths, about the same as in the rural districts of the coast. This mortality is exceeded in the Gulf coast, South Mississippi, and southwestern plains and plateaus, so that it is the region of a less fatal prevalence of the malarial fever than any other in the malarial region.

The individual answers to questions state that malarial diseases now prevail to a less extent than formerly in this group. Nine say that the prevalence is the same as formerly; only one or two suggest that it is more prevalent.[1] Throughout North Carolina, South Caro-

[1] The mountain regions of the States in this group are not included in this place; they were considered in Region I. (non-malarial).

lina (with one exception), and Florida (with one exception) malarial diseases are reported as less prevalent now than formerly.

The belief in the existence of a continued malarial (bilious or remittent) fever is here almost universal. This fever, in its most characteristic form, lasts five, seven, or fourteen days, and for its symptoms has a yellow, coated, large tongue, an irritable stomach, constipation; the onset of fever is sudden with chills, the temperature rises rapidly to a high point, has distinct remissions, with sweats. It is directly and favorably influenced by the quinine treatment in full doses, is rarely fatal, and is followed by a rapid convalescence.

The belief in the occurrence of a fever which conforms neither to the type of malarial nor to that of typhoid fever, and which results from the blending of the features of the two diseases, is very general in this region. Rather more than half of those who have replied to this question express the belief in the existence of a *typho-malarial fever*. Some express a quasi-belief, or admit their inability to decide the question. Nearly one-half believe that the two diseases always appear distinctly differentiated by their symptoms; that all fevers which occur are either typhoid or malarial fever; and that there is no such disease as typho-malarial fever. The argument here adduced for the belief in typho-malarial fever is entirely a symptomatic one; it is not based upon post-mortem study, because typho-malarial fever is rarely fatal. And it will now be seen what the symptomatic features are which lead to the recognition of typho-malarial fever as a distinct disease.

The symptoms of typho-malarial fever, as they are reported to me, can be arranged under three heads:

a. Symptoms given as indicative of typho-malarial fever, in which there is more or less resemblance to the symptoms considered characteristic of malarial fever.

High temperature in the onset generally preceded by chills.

Irregular or repeated exacerbations of fever.

Marked remissions, but less distinct in proportion to the intensity of the fever.

Gastric irritability. Vomiting of bile.

Hepatic congestion. Jaundice. Splenic enlargement.

Constipation.

Aching pains in the extremities and spine.

Absence of rose spots.

Tympanites.

Sometimes delirium.

Fever process not controlled by anti-malarial treatment.

Lasts from three to six weeks.

Rare fatality.

b. Symptoms reported as indicative of typho-malarial fever, in which many of the symptoms more nearly resemble those of typhoid fever than of malarial fever.

Sometimes chills. Rapid rise of fever.

Morning remissions.

Tongue dark brown in middle, red on edges, and dry.

Tympanites, tenderness, and iliac gurgling.

Diarrhœa.

Intestinal hemorrhage.

Bronchial catarrh.

Muttering delirium.

Rapid decline of fever with profuse perspiration.

c. Symptoms in early stage of illness indicating malarial disease, later assuming the character of true typhoid fever.

Another variety of the group is said to have an onset like that described, with marked matinal remission and perspiration; after a few days the disease "assumes a typhoid type;" the fever becomes continued, and the more characteristic symptoms of typhoid appear.

These three divisions contain the symptoms which are considered to form the basis for the diagnosis of typho-malarial fever. In the first division the grounds for the theory of a typhoid element must rest upon the facts of the long duration of the fever, notwithstanding the administration of quinine in antipyretic doses.

In the second division the evidences of a malarial influence are the sudden onset with chills, the more rapid rise of fever, the remissions, which are sometimes marked, and the rapid decline of fever with perspiration.

In the third variety the onset with exacerbation and decided remission with perspiration is the only peculiarity.

It has been stated that many observers in this region do not accept typho-malarial fever as a distinct pathological entity. One says: "The disease which some physicians call typho-malarial fever is, I am satisfied, nothing more nor less than continued malarial fever. . . .

In my experience, where typhoid symptoms occur in all cases, genuine typhoid fever is present, and the malarial affection must be excluded."[1] Other observers state that they have never seen cases which partook, at the same time, of a malarial and typhoid element, and that in cases thus recognized, and in localities in which the existence of regular typhoid fever is not thought to appear, the characteristic typhoid lesions of the intestinal glands have been found;[2] and many assert that all cases which they see can be classed as enteric or as malarial fever.

"The cases of fevers which prevail here (Brunswick, Ga.) are not well-defined cases of typhoid fever, but bear sufficient resemblance to true typhoid in duration, to be classified with it rather than with true malarial continued fever, over which medicine, as to duration, usually exercises a controlling influence." This fever has sometimes rose spots; but the symptoms most commonly met with are slight tympanites, constipation, slight bronchial catarrh, and delirium (not severe) at some stage. The spleen is not at all enlarged, or slightly so; there may be hemorrhage from the intestines. The duration is two to four weeks; the most usual being four weeks. These cases are generally called typho-malarial, and when of short duration are said to be bilious remittent.[3]

Atypical Forms of Continued Fever; Unclassified.—Observers in this region report the prevalence of a fever of a negative character which has neither the symptoms characteristic of typhoid nor of malarial fever. This fever is said to have no intermissions nor remissions, and generally quinine has no effect. Yet the only symptom resembling typhoid is the continued fever. The tongue, in some of the cases, will remain normal; the appetite fair; bowels regular; no spots; still a temperature from 102° to 103.5° F. will persist for twenty days or more. The majority of these continued fevers will have one or two features of typhoid.[4]

Another correspondent recognizes "a distinct type of continued fever differing pathologically from both typhoid and malarial fevers. It occurs where malarial fevers are unknown in these pronounced forms; is not amenable to quinine; exhibits no splenic enlargement; no marked tendency to periodicity. It is not attended by rose-spots,

[1] Seneca, S. C., Dr. Robert C. Davis.
[2] Albany, Ga., Dr. P. L. Hilsman.
[3] Dr. Robert Hazlehurst, Brunswick, Ga.
[4] Gainesville, Fla., Dr. N. D. Phillips.

diarrhœa, abdominal distention, hemorrhage from the bowels, nor, as a rule, delirium."[1]

Cases of continued fever of this character, lasting from ten days to three weeks and longer, which are not thought to be typhoid, because they do not have the characteristic symptoms of enteric fever, and not believed to be malarial because they occur where malaria is absent, have none of the characteristic symptoms of malarial disease, and resist the action of, or are aggravated by, quinine.[2] The highest temperature is not infrequently in the beginning of the attack; it pursues a regular course with slight morning remissions and evening exacerbations, but with a gradual daily decline until normal temperature is reached.[3] Recovery is the almost invariable result. In these negative cases an autopsy has revealed sometimes the lesions of typhoid fever, much to the surprise of the physician.[4]

Another correspondent says, "We have a continued fever (not malarial) very prevalent at times, and there is a general tendency to call this fever typhoid, but there is an absence of the characteristic symptoms of typhoid, as rose-colored spots, tympanites; constipation is generally the rule; and there is a great tendency to relapse, particularly when there has been any error in diet, the relapse often proving fatal. This fever is called typhoid by many practitioners, particularly after it has proved fatal. Quinine has no effect upon it, save when in large doses at the commencement, when it depresses the fever. It is not complicated with malaria as a rule."

A continued fever has been described by Dr. Porcher, of Charleston, as "gastric remittent," or "irritative fever;" there is no malarial element in it, and it is not influenced by quinine. It is distinguished by a red pointed tongue, and is seldom, if ever, fatal if recognized and properly treated.

Second Group

CLASS I.—The Census of 1880 shows that in this group the ratio of deaths from typhoid fever averaged 27.7 per 1000 deaths. This is higher than in Region I., which had a death-rate of 24.03, and higher than in Region II., which averaged 17.3. The high rate, however, in this group was due to the ratio of 41.9 in Central Alabama,

[1] Columbia, S. C., Dr. A. N. Talley.
[2] Greensboro, Ga., Dr. J. H. Gheesting.
[3] Elizabeth City, N. C., Dr. J. E. Woods.
[4] Tampa, Fla., Dr. John P. Wall.

and Central and Northern Mississippi. For the Gulf Coast Region proper the death-rate was only 7.7 in the urban, and 30.6 in the rural population. The Gulf Region, therefore, furnishes a lower death-rate, 19.1 (combining the city and rural ratio), than is to be found in any other of the regions—the ratio in cities of 7.7, being lower by 2.4 than the next lowest ratio of 10.1 in the Middle Atlantic Region. A similar conclusion can be drawn from an estimate of the number of cases of typhoid fever in this region as compared with the total cases of disease. According to the statistics of the Marine-Hospital Service, the percentage of enteric fever to all cases admitted here was 0.1. In one other region—the Mississippi—the percentage was the same, but in all the others it was higher, being 2.05 in the North Atlantic, and 0.3 in the South Atlantic Region.

The belief in the rare occurrence of typhoid fever, or of the assemblage of symptoms usually recognized as necessary to the diagnosis of typhoid fever, is almost universal in this subdivision. Such replies as: "in fourteen years I saw only two cases of what I consider real typhoid fever;"[1] "I have treated about only a dozen cases in nineteen years;"[2] "in a practice of twelve years I have never seen a typical case of typhoid fever;"[3] are frequent in the reports from Alabama. By some the disease is said to have undergone a change to a mild type, "none of the typical symptoms is found generally present."[4] In Mississippi the reports are to the same effect. "Text-book typhoid fever is rare, or unmixed typhoid is not believed to exist." "In an experience of thirty-five years I have never seen a true type of typhoid fever as described in the books;"[5] "I have never seen but one typical case of typhoid fever during my eight years' practice."[6] "There is no typhoid fever in this vicinity. I have seen one case in thirteen years."[7] "In an extended practice of eighteen years I have met with what I consider no typical case of the above fever as described by Louis."[8] "In twenty-five years' practice I have met with only six cases of typical typhoid fever."[9]

In Louisiana the disease is still more rare, as the following testimony shows: "I do not believe I have seen, on an average, one a year for the last thirty-three years."[10] "I have never met with a case

[1] Tuscumbia, Ala., Dr. E. P. Rand.
[2] Gaston, Ala.
[3] Troy, Ala.
[4] Camden, Ala., Dr L E. Starr.
[5] Grenada, Miss.
[6] Black Hawk, Miss., Dr. J. W. Jordon.
[7] Durant, Miss., Dr. R. B. Carsen.
[8] Vicksburg, Miss., Dr. R. A. Quinn.
[9] Bay St. Louis, Miss., Dr. A. Parker.
[10] Shreveport, La., Dr. Thos. J. Allen.

contracted here, in a continuous practice of twenty years in different localities in North Louisiana." "I have seen one case in a native in three years in a total of 4500 patients."[1] "I have observed during seven years about 20 cases presenting the pyrexial nervous and abdominal symptoms of typhoid fever." "It is not a generally accepted doctrine among the physicians of subtropical and tropical regions that typhoid fever exists in such localities at all. In Southwestern and Southern Louisiana it may exist in a sporadic form;"[2] as post-mortem and clinical studies have clearly proven. "I have never seen a genuine case of typhoid fever in the State, except cases imported from some other State, or from across the water."[3]

There are two sides to this question, and it is not admitted by all that typhoid does not exist, or that it is very rare. In South Louisiana, where it is thought by some never to appear, it occurs among the natives, and its typical lesions are found in fatal cases.

As regards the symptoms, twenty-five say that rose-spots are not usually present, and thirteen have the opposite opinion. Tympanites and diarrhœa are the only symptoms upon which a majority unite as common. There is an exact division of numbers in deciding as to the usual presence or absence of bronchial catarrh and delirium. Only four have met frequently with intestinal hemorrhage; enlarged spleen has been more frequently seen, but it is not a usual condition. Those who give percentages say that rose-spots occur in 10 or 15 per cent. of cases; tympanites in 20 per cent.; diarrhœa in 50 per cent.; bronchial catarrh in 50 per cent.; delirium in 20 per cent.

The usual duration is stated to be from three to four weeks, the latter being somewhat more common. Some cases last 5 weeks, a few 8 or 10, and a duration of one week has been noted by one physician. Relapses are very rare; seven only report that they are common. The percentage of fatal cases is given by different observers as follows: One places it 0 per cent.; one at 2 per cent.; one at 3 per cent.; one from 3 to 5 per cent.; two at 4 per cent.; three at 5 per cent.; one at 5 to 10 per cent.; one at 6 per cent.; one at 8 per cent.; two at 8 to 10 per cent.; three at 10 per cent.; one at 16 per cent.; three at 20 per cent.; one at 20 to 25 per cent.; one at 33 per

[1] New Orleans, La., Dr H. B. Carter.
[2] New Iberia, La., Dr. Hebert.
[3] Ruston, La., Dr. F. M. McCormick.

cent.; one at 40 per cent. (Jackson, Miss.); one at 80 per cent. (New Orleans).

The most frequent cause of death was exhaustion. The typhoid fever of this region may be said, therefore, to be a continued fever of quite limited prevalence, and not increasing, having no great intensity in its manifestation. Its chief symptoms are a continued febrile movement, with diarrhœa and tympanitic distention of the abdomen; rose spots are not seen in most cases. Enlargement of the spleen is rare, and the graver complications, as intestinal hemorrhage and perforation, are still more rare. The attack lasts from three to four weeks, and relapses are quite uncommon. Death occurs in a small percentage of cases; the smallest death-rate in the United States from this disease being found in this group.

Class II. *Malarial Fever; Typho-malarial Fever, etc.*—Fevers of paludal origin are diminishing in this group. To this statement there are eighteen contrary assertions; nine that this prevalence is the same; and nine that it is greater than formerly. They have, however, a very marked general distribution. The census of 1880 shows the high mortality in this group of 75.04 in 1000 deaths, excluding the cities of the Gulf Coast; in the cities it is 60.9. The highest mortality anywhere in the group is 85.6, in the lower Mississippi Region.

The ratio of prevalence of malarial disease in the U. S. Army was much higher in this group than elsewhere; the Gulf Coast having a ratio of 304.05, and the region north of the coast of 512.37 in 1000 effective force.

In the Gulf District there were 513 cases of malarial disease in a total of 2775 cases in the Marine-Hospital Service in 1876, and 1174 malarial cases out of a total of 5011 in 1887, a percentage of 18.4 in 1876, and of 23.4 in 1887.

Remittent fever is believed to be a common form of fever in this group, and to be readily differentiated from typhoid fever; there are sixty-two answers in support of this opinion; seven answers are partly negative. It is said by some to constitute the greater part of all the cases of continued fever; by some to be the *only form* of continued fever prevailing.[1] The negative answers do not say that remittent fever does not prevail, but that it is of such short duration and so gen-

[1] Gaston, Alabama.

erally and quickly cured by quinine, that it does not merit the name of continued fever, and can never be confounded with enteric fever.

As there is so little recognized typhoid fever in this group, the comparisons are generally made between remittent and typho-malarial fever; the latter is distinguished by having a higher temperature, with a greater tendency to gastric disorder and cerebral congestion, and a greater fatality. It does not usually resemble typhoid in any particular except in its duration.

Sometimes "malarial remittents have some of the symptoms of typhoid fever," and more than one report says that the prevailing fever in the group is adynamic remittent fever resembling typhoid, but distinguished from it by the absence of rose spots and other pathognomonic symptoms; tympanites and sometimes diarrhœa occur. Doubt is expressed sometimes as to whether this fever is typhoid fever complicated by malaria, or malarial continued fever of adynamic type.

In regard to the existence of a distinct type of fever which is neither malarial nor typhoid, conclusions are equally divided in favor of and against this theory. Nearly one-half of the replies assert that typhoid fever is modified by the malarial poison, or that there is a continued fever to which the name typho-malarial can be properly applied. About one-half of the correspondents say that all the continued fevers which they see are either malarial or typhoid, and that there is no hybrid or blended disease. Others contend that typhoid is complicated by malaria, not changing the type or giving it any new nature.

The faith in typho-malarial fever is a confident one, and is based upon the coincidence of symptoms which are found in typhoid and in malarial fever, but the symptoms are not aggregated in any fixed number or association. Various groups of associated symptoms are given by correspondents.

Groups of Symptoms given as Indicative of Typho-malarial Fever:

1. Headache.
   Pains in spine and extremities.
   Anorexia.
   Marked remissions in fever.
   Vomiting of bile—jaundice—enlargement of liver.

Iliac tenderness; gurgling.
Tympanites.
Rose spots.
Carphology—subsultus tendinum.
Early prostration.

2. Rapid rise of fever, remissions, and exacerbations.
Tympanites.
Constipation or diarrhœa.
Intestinal hemorrhage.
Muttering delirium.
Rapid decline of fever, with profuse perspiration.

3. Continued fever, morning remissions.
Gastric irritability.
Hepatic and splenic enlargement.
Absence of rose spots.
Constipation.

4. Continued fever, with a course resembling typhoid fever.
Constipation.
Absence of rose spots, tympanites, diarrhœa.
Nervous disorders.
Death rare.

5. The only distinction made by some is based on the height of temperature. "A temperature of 103° in the forenoon to 104° or slightly higher in the afternoon . . . we call typhoid fever, while (and these are much more numerous cases) if the temperature ranges under, and especially if much under, the degree noted, we call it typho-malarial fever." [1]

But few records of autopsies in cases of malarial fever have been received. "In the few cases in which autopsies have been held, the lesions are very similar to those in typhoid fever; hemorrhage from the bowels has been a fatal symptom in some cases in which autopsies have been denied." [2]

The various grouping of symptoms is an evidence of the difference of opinion as to what constitutes a typho-malarial fever in the experience of different observers. There is no difficulty in believing that a modification of the typhoid state by malaria, or of a malarial basis

[1] Union Springs, Alabama, Dr. C. H. Franklin.
[2] Shreveport, Louisiana, Dr. Thomas J. Allen

by typhoid infection, may present itself in a very great range of intensity and with every variety of associated symptoms. The argument to disprove the existence of typho-malarial fever as a distinct pathological entity, must be based upon our present knowledge of fever processes and on pathological anatomy, and must determine whether there is a necessity to sustain a separate place for such cases, or whether they can be referred to typhoid fever or adynamic malarial fever.

The believers in typho-malarial fever are nearly evenly balanced by those who do not accept the hybrid or combination theory of nature or symptoms. Such replies as, "there is no such disease as typho-malarial fever," are very common in this group. "If I thought so," one physician says, "I would treat these cases with quinine, thereby eliminating the malarial element and converting the case into one of pure typhoid."[1] "The term is a misnomer."[2] "I am not sure that such a fever exists."[3] "There is no such disease as typho-malarial fever. I have vainly sought for a single case of the disease."[4]

Atypical Typhoid Fever; Simple Continued Fever; Thermic Fever; Fevers of Doubtful Nature.—The fevers so far considered have some right to the titles given them in the presence of features more or less resembling typhoid or malarial fever, separate or in conjunction. This is true of all the varieties mentioned, except those included under the fourth and fifth groups of symptoms, called typho-malarial fevers. These were negative pictures which are called typho-malarial because they resemble neither typhoid nor malarial, the designation implying that the infection by two poisons brings about a neutralization of their virulence, and that each subdues the active hostility of the other.

Fevers of this negative character are called by other names, however, in this group of States. One form constitutes the prevailing fever in this locality, so that if a name and a pathology could be found for it, the proper classification of all the forms might be considered as finished. The typical typhoid fever being fixed by its rose spots and associated nervous and intestinal symptoms; typical malarial remittent

[1] Tuscumbia, Alabama, Dr. E. P. Rand.
[2] Greensborough, Alabama, Dr. William C. Avery; and Tuskegee, Alabama, Dr. William J. Gautier.
[3] New Iberia, Louisiana, Dr. Hebert.
[4] Natchez, Mississippi, Dr. N. L. Guice.

by its short duration, exacerbations and remissions of body-heat and sweats, and by its subjection to the quinine treatment; and typical typho-malarial by the combination of the more typical symptoms of both of these. There is but one other form to be classed, and that one the *most diffused and the least fatal in the Gulf Region, especially in Louisiana.*

It is very clear that those who speak of typhoid fever as being rare in this locality refer only to the well-defined cases with characteristic symptoms, but there is a large class called by various names, as in Louisiana, Louisiana fever, atypical typhoid fever or mild malarial continued fever, which prevails in the autumn, lasts from two to four weeks or longer, and has as its chief symptom continued fever, with slight morning remissions (the morning rise being higher than evening, in some cases ranging from 101° to 103.5°), with no eruption or intestinal symptom, without nervous manifestation, and often without loss of appetite throughout the attack. The tongue is white or dark coated, not red and pointed. The patient does not have the facial expression of serious illness, as in enteric fever, nor the flushed face of remittent. There is no aching in the back and joints, or extremities. Convalescence is very tedious, often marked by great and prolonged debility, "the patient has to learn to walk," and is not well for weeks or months.[1] This fever prevails in Alabama, Mississippi, and Louisiana.

That careful and accomplished observer Dr. Guitéras[2] has made a study of fevers of this group which "are characterized by the existence of fever and the absence of specific symptoms," and has found them to prevail where there is no malaria and no enteric fever, and he concludes that they are due to a "disturbance of the nervous apparatus which controls the heat functions of the body," the exciting cause being the prolonged and excessive heat of semi-tropical countries. The temperature charts of these cases do, indeed, show that the temperature variations are not like those of typical typhoid fever, but they do not differ materially from many cases of enteric fever which we see in other regions. The autopsies which he has recorded are too few to base any conclusions on, and although his facts have weight, they are not yet numerous enough to lead to the adoption of his views.

[1] Camden, Ala., Dr. L. E. Starr; Tuscumbia, Ala., Dr E. P. Rand; Coalburg, Ala., Dr. J. M. Armstrong; Birmingham, Ala., Dr. T. L. Robertson; Tuskegee, Ala., Dr. Wm. I. Gautier; New Orleans, Dr. H. R Carter, Dr. W. Page McIntosh, U. S. M.-H. S.

[2] U. S. M.-H. S. Report, 1885.

The conclusion from the study of the fevers of the Gulf States is that typical typhoid fever is rare, that malarial fevers are rife, and that atypical, mild continued fever, without characteristic symptoms, is a very prevalent form of disease.

Third Group.

This group includes the region of the southwest central plains and plateaus, with portions of the prairie region and the western plains, and covers the State of Texas (except the coast region), Middle and Northern Louisiana, Arkansas, Lower Missouri, all of Kansas, and part of Kentucky and Tennessee.

CLASS I.—The report of the U. S. Army for 1887 shows that the highest admission rate for typhoid fever in the year was in this region at Fort Davis, Texas. It was 49.8 per 1000 of mean strength. The second was also in this group at Fort Riley, Kansas, of 25.1; the third at West Point, N. Y., 21.1; and the fourth, just north of this group, at Jefferson Barracks, Mo., of 15.6.

This group has furnished me with a large number of very interesting papers, giving a statement of facts and opinions, which would require a very lengthy and thorough analysis to do them justice and to extract from them all the valuable testimony which they contain.

Typhoid fever, in its typical character, is believed to be a rare disease by the large number who have replied on this point. Fifty-five say that it is limited in prevalence or very rare—twenty-seven only admit any extended prevalence. Such statements as "have seen four cases in fourteen years," "three in nineteen years," "one in six years," "two in seven years;" "it forms five per cent. of all fevers," or "two per cent. of all fevers in fifteen years' practice;" "we seldom, if ever, have true typhoid fever."[1] "We do not have genuine typhoid fever in this locality."[2] "We have no typhoid fever here."[3] "Have never seen a case of enteric fever in this section. We have a low form of continued fever but it possesses but few of the characteristic symptoms of enteric fever."[4] He does not give a name to this fever.

[1] Greenville, Texas, Dr. S. R. Dunn.
[2] Sulphur Springs, Texas, Dr. E. P. Benton.
[3] Hillsborough, Texas, Dr. U. B. Kennedy.
[4] Waco, Texas, Dr. R. W. Park.

"We have no typhoid fever in this section; nor have we had since 1870, in which year it was imported by immigrants from Georgia." "The prevailing fever has many symptoms resembling those of typhoid." "We never have rose-colored spots, and seldom, if ever, have the low muttering delirium . . . the temperature is out of all proportion to the pulse. The temperature may be 104.3°, while the pulse-rate will be as low as 60; indeed, I have seen it as low as 40." "There is also less pain in head, no diarrhœa, none of the ochre-colored stools. It lasts twenty-three to thirty days, may continue eight or nine weeks attended by symptoms of mental disturbance." [1]

"I have been doing an extensive practice for eleven years and have seen no case of genuine typhoid fever during that time." [2] All fevers are malarial in nature, there is a "continued malarial remittent." "It is of very rare occurrence. A typical case in this section is almost a curiosity. Cannot classify the prevailing continued fever." [3] "In eight years of practice here, have seen six cases, four hospital cases, seamen from Philadelphia, of which two died; among residents of city, not a typical case." [4] "Do not have any genuine typhoid fever here, malarial fevers are the chief febrile diseases we have." [5] "Have seen but two well-marked cases in a practice of seven years." [6] "I have not seen a typical case of typhoid fever for several years." [7]

It is an invariable rule that where typhoid fever is said to occur rarely the physician states that he "does not pronounce a case typhoid unless it is nearly a typical case." [8]

Views of prevalence differ in the same localities. Where it is said by some to be "rare," by others it is said to prevail extensively with characters sufficiently well defined to render the diagnosis possible. There is not so much difference as to facts but as to their interpretation, as to what is the degree of typical manifestation which justifies the diagnosis of typhoid fever.

There are thirty-two replies to the effect that typhoid fever is more widespread than formerly, and forty-five that the cases have not recently increased in number. It not only is said to have become more prevalent, but the cases are now typical, as in Kentucky and

[1] Whitney, Texas, Dr. R. G. Williams.
[2] Eagle Lake, Texas, Dr. F. O. Norris.
[3] Galveston, Texas, Dr. W. C. Fisher.
[4] Galveston, Texas, Dr. Henry P. Cooke.
[5] Houston, Texas, Dr. W. A. Archer.
[6] Girard, Kansas, Dr. George W. Miller.
[7] Madison, Kansas, Dr. L. J. Cunkle
[8] Paducah, Ky., Dr. J. G. Brooks.

Kansas.[1] Eighty-one regard the prevalent form as having a moderate or mild intensity; while twenty-seven think the type is severe.

The cases recognized as typhoid fever in these enumerations are said by fifty-three to have most of the typical symptoms; twenty say that the disease is not well defined. The symptoms most commonly met with are tympanites, diarrhœa, bronchial catarrh, and delirium; there is a great unanimity of opinion on these points. Rose spots are not usually seen in the opinion of forty-seven, while twenty have the contrary view. A small majority think that enlarged spleen is not common; intestinal hemorrhage is said by fifty to be uncommon, and by twenty to be frequently met with. The actual figures cover a wide range. The percentage of the rose rash runs from 10 to 50; tympanites from 33 to 100; diarrhœa from 30 to 50; intestinal hemorrhage from 3 to 10; enlarged spleen from 30 to 40; bronchial catarrh from 12 to 50; delirium from 3 to 80. But the majority give tympanites and diarrhœa a percentage of over 50; bronchial catarrh and delirium over 25; enlarged spleen over 30; and intestinal hemorrhage under 10; the percentage of rose spots is given below 30. The duration is given at three, four, or five weeks; one-third have fixed upon each of these periods, so that opinion on this point is about equally divided. The shortest duration is said to be two weeks, and the longest eight to ten weeks. Relapses are not common; if we accept the assertion of nearly sixty physicians, only eight believe they frequently occur.

The fatality of the disease is said to be low. The actual figures range from 3 to 66 per cent.; but the larger number of percentages are below 10, and one-half of these below 5.

A considerable portion of this area—the southwest central plains and plateaus—has, in the census year of 1880, the very high ratio of 48 in 1000 deaths from all causes; this is higher than in any other group which is represented. The next highest death-rate is 46.7, in the south-central Appalachian region—a part of the non-malarial group—and covering the mountain area of Pennsylvania, West Virginia, Virginia, Tennessee, North Carolina, North Georgia, and Alabama. It is higher than the South Atlantic and the Gulf Coast, which is 30.6, and that of the greater part of South Carolina, Georgia, Alabama, and Mississippi, which is 41.9.

[1] Wichita, Kansas, Dr. H. H. Fabrique.

The cause of death is stated, in more than fifty reports, to be exhaustion of the heart, or general exhaustion from fever, etc.; intestinal hemorrhage and perforations are not infrequent; other causes are pneumonia, hyperpyrexia, cerebral congestion.

CLASS II.—*Malarial fevers* prevail to a great degree throughout this area. But that they are not as widely spread as formerly is the opinion of the largest number. Twenty say they are more prevalent; thirteen that they are the same as formerly. As malarial fevers disappear, typhoid increases; this is a common statement. "Our fevers were almost altogether of malarial character, but seem to be growing less, and typhoid fever is becoming more prevalent."[1] We have less paludal disease now than formerly, is an oft-repeated answer. As a sample of this, I will quote as follows: "We have much less malarial fever in this country than is generally supposed, even by practitioners of medicine, who thus unnecessarily use large doses of quinine."[2] *Per contra*, it is said "that the majority of our fevers are malarial. Probably eighty per cent. of the prescriptions contain quinine, and about as large a per cent. of our sickness is benefited by quinine."[3]

To the question as to whether a continued bilious, or remittent malarial fever was recognized which could be differentiated from typhoid fever, there were sixty-nine affirmative and three negative responses. The answers in the negative came from Eastern Texas, and Louisville and Danville, Kentucky.

The simplest form of malarial fever described by correspondents is one lasting from six to fifteen days, distinguished by rapid rise of fever to a high point, with decided morning remissions, headache, and pains in the limbs, tongue thick, with a white coating, thirst, anorexia, epigastric tenderness, with vomiting and usually constipation. Quinine makes a decided impression and shortens the attack. Other definitions and descriptions are: "A continued fever, 'with liver and splenic complications,' is called a malarial continued fever." "Malarial fevers often assume a typhoid character." "A continued malarial fever prevails in Texas and elsewhere, which is called by common consent typhoid." "Some physicians diagnosticate typhoid, but such cases do not present the diagnostic symptoms of typhoid fever."[4] This form "has slight

[1] Clinton, Kentucky, Dr. George Beeler.
[2] St. Louis, Missouri, Dr. P. G. Robinson
[3] Wichita, Kansas, Dr. J. S. Foote.
[4] Jefferson, Texas, Dr. A. A. Terhune, Sr.

elevation of temperature in the morning, with an evening exacerbation. It runs its course in from one to eight weeks, and ends in recovery."[1] "The absence of rose spots, iliac tenderness, and diarrhœa distinguishes it; the other symptoms, enlarged spleen, bronchial catarrh, delirium sometimes exist."[2] "A great many so-called cases of typhoid fever are nothing but continued malarial fever."[3]

The existence of a continued malarial fever is absolutely denied by some in cases in which quinine has no effect. "I do not believe" one correspondent says, "we ever meet with a continued bilious malarial fever, after the administration of quinine, without abating fever. I regard it as modified typhoid."[4]

Is typhoid fever modified by malarial infection? is answered by thirty-six in the affirmative and by twenty-six in the negative. Seven think such a modification is a rare occurrence; and seven are in doubt and unable to answer the question. Some correspondents say that all typhoid is modified by malaria. Those who admit such a modification do not say that it always exists in any given locality; it "occurs in some cases," or "to a limited extent." The modification by malarial influence is not thought to aggravate the typhoid intensity, but, on the contrary, to lessen it. It is said not to be so fatal because thus modified.

In other replies enteric fever is thought to be aggravated by malarial complications. This modification, or complication, is thought not to extend beyond the first week, to be shown by remissions at the time, and to disappear because checked by quinine, or because the disease passed into a continued typhoid fever proper. In convalescence, the malarial complication is said to appear and to be quickly relieved by quinine. The belief in this modification is often a part of the general belief that all diseases, of whatever nature, are influenced by malaria and require quinine treatment.

There is a very general and fixed conviction that there is a distinct type of fever called *typho-malarial*, but the most conflicting meanings are attached to this designation.

1. It is applied by some to mean "a *continued malarial fever* with typhoid symptoms, which is not controlled by the same means, or in the same limited time that usually suffices in reducing remittent fever."

[1] Hillsborough, Texas, Dr. N. B. Kennedy.
[2] Clebourne, Texas, Dr. W. M. Yoler.
[3] La Grange, Texas, Dr. J. C. B. Renfro.
[4] Waco, Texas.

This form is marked by remissions, sometimes by intermissions. This is, of course, not a distinct type of fever, though the prefix *typho* is used as an adjective, and does not mean a blending of the two specific conditions of disease.[1]

2. "This fever is nothing more or less than a profound malarial poisoning in a case in which the vitality is either greatly reduced by the intensity or continuance of the attack, or by previous conditions . . . in which the system is badly run down."[2]

3. To others it means a fever which "begins in all respects as a malarial intermittent. After a few days it becomes continuous in spite of anti-periodic remedies, and then eventuates in a more or less typhoid of true type."[3] It is a "continued type of fever with exacerbations and remissions (frequently on alternate days) showing a greater range of temperature than that of typhoid—beginning abruptly, malarial tongue, intestinal symptoms, tympanites . . . does not yield to quinine; lasts twenty to twenty-eight days."[4] A fever "commencing as bilious remittent fever, yielding to proper treatment by quinine in six or seven days, then relapsing, in from three days to a week, into a continued fever that quinine aggravates, and assumes the symptoms of typhoid fever."[5] "Have seen a few cases which commenced as a severe form of intermittent fever, the temperature rising the second day to 102° or 104.5°, which under the influence of large doses of quinia would drop to 101° or 102°, then run a typical course of typhoid fever, with intestinal hemorrhage or perforation."[6]

4. By others it is said to be a fever "having more of the characteristic 'landmarks' of *typhoid fever* than of malarial fever, as tympanites, intestinal hemorrhage, etc."[7] A fever "commencing with chill, after which the fever runs high; after a few days, tympanites, parched tongue and lips, tenderness over right iliac fossa, delirium, subsultus tendinum, but constipation in most cases."[8] "A continued fever with temperatures (102°–104°) higher at night than morning, often pulse not in harmony with the high temperature. Patient dull, seldom delirious, tongue coated persistently; some tympanites, patient wants to be let alone; fever runs twenty-eight days

[1] Bowie, Texas, Dr. W. G. Hayes. Eagle Lake, Texas, Dr. F. O. Norris.
[2] Savanna, Indian Territory, Dr. E. Poe Harris.
[3] Sherman, Texas, Dr. J. B. Stinson.
[4] Rice, Texas, Dr. J. A. McGill.
[5] Jefferson, Texas, Dr. A. A. Terhune, Sr.
[6] Leavenworth, Kansas, Dr. W. W. Walter.
[7] Dallas, Texas, Dr. I. M. Pace.
[8] Delphos, Kansas, Dr. C. C. Surber.

and neither quinine nor anything else has any effect, beneficial or otherwise; almost all got well. If they die, death occurs within four or five days. Have not had an autopsy."[1] A fever which is "a compound of both" (malarial and typhoid) "being at first malarial; pain in the stomach and constipation, and absence of typhoid symptoms; later it is marked by the rose spots, diarrhœa, and general typhoid symptoms, but rarely, hemorrhage."[2] This same opinion is expressed by other correspondents.[3]

5. The name typho-malarial is also applied to a class of fevers lasting fifteen to twenty-eight days, with an evening temperature averaging 103.5°, a morning temperature of 101.5°; pulse 106 to 115; bowels and kidneys not affected; appetite good until five to eight days, then capricious; no pain, rarely headache, occasionally slight diaphoresis; no eruption, no delirium, no tympanites, no tenderness, considerable prostration in third week" . . . and having a small mortality.[4] "No epistaxis or hemorrhage from the bowels; no rose spots. Convalescence more rapid and relapses very rare."[5]

This is the same fever spoken of as a distinct type by Dr. R. W. Park, of Waco, Texas; Dr. R. G. Williams, of Whitney, Texas.

Those who reply in the negative say that they "have never seen a case which they could recognize as typho-malarial fever. They believe the so-called typho-malarial fever to be a mild form of typhoid fever which . . . is free from the complications of severe typhoid fever."[6] "There is a typhoid fever which has a specific course. There is also a malarial fever which has a specific course; they may prevail at the same time in the same individual, under which circumstances I denominate the case one of typhoid fever and malaria."[7] "The cases that are called typho-malarial in this section can be divided into mild cases of typhoid and severe remittent."[8] "I recognize no compound of typhoid and malarial fever, but consider them incompatible."[9] "I frequently see a continued malarial fever of an intermittent character but not at all resembling typhoid fever. I have regarded all the cases of continuous fever lasting from three to five weeks as typhoid fever." He does not believe there is any distinct fever which

[1] Wichita, Kansas, Dr. I. S. Foote.
[2] Assaria, Kansas
[3] Lawrence, Kansas, Dr. F. D. Morse.
[4] Sherman, Texas, Dr J. F. Wilson.
[5] Greenville, Texas, Dr. I. R. Dunn.
[6] Hopkinsville, Kentucky.
[7] Louisville, Ky., Dr I. A. Larrabee.
[8] Newcastle, Ky.
[9] Olmstead, Ky., Dr I. R. Bailey.

is neither malarial nor typhoid.[1] "I consider what is called typho-malarial fever a milder form of typhoid fever."[2]

A mild type of continued fever, without the more characteristic symptoms of malaria or of typhoid fever, and often with no well-defined symptom except continued fever, is found in this territory; it is called atypical or mild typhoid fever, continued or simple continued fever, obscure or undetermined fever, and fever of locality and catarrhal fever. It is said to have a very extensive distribution. The following notes show its chief features.

"A distinct type of continued fever, which differs greatly from enteric fever. I do not think it is 'malarial.' The characteristic symptoms are—low fever, temperature ranging from 99° to 102.5°; constipation (usually), loss of appetite, languor and debility, tongue usually moist and clean or only lightly furred."[3] "The temperature is often higher in the morning than in the evening. The patient is often constipated; there is no tympanites, no rose-color spots, no delirium; fever may last for four weeks."[4]

This fever "is neither typhoid nor malarial, neither is it typho-malarial fever. The only resemblance to typhoid is that it is continuous and usually terminates in twenty-one days. No tympanites, no dryness of tongue, no diarrhœa, but often constipation. Rarely ever much delirium. Temperature ranges from 101° to 103° A. M. and from 102° to 104° P. M. Prostration and sometimes extreme emaciation occur, but never fatal. Antiperiodics have no effect, except to relieve nervous headache."[5]

"Typhoid fever is a rare disease in Galveston, and with a population of 40,000 inhabitants I doubt extremely if we have twenty-five cases a year. As resident surgeon of St. Mary's Infirmary from 1882 to 1885, an institution with an average attendance of one hundred and twenty-five, I do not think I saw during that time four well-defined cases of typhoid fever. But during this time we had quite a number of cases suffering from what we called 'continued fever;' some regard it as catarrhal in its nature, others as a mild form of typhoid, and others are honest enough to confess that they do not know what it is—I belong to the last class. It lasts from seven to forty days, and

[1] Leavenworth, Kansas, Dr. Van Eman.
[2] Dancyville, Tenn., Dr. T. K. Powell.
[3] Waco, Texas, Dr. R. W. Park.
[4] Waco, Texas, Dr. O. J Halbert.
[5] Nicholsville, Ky., Dr. Charles Mann.

the rule is for the patient to recover. The patient is not often delirious, sometimes has slight diarrhœa, oftener constipation, rather clean tongue, slow pulse, moist skin, and a temperature of 101° to 102°; at times lower and then higher than this, with half a degree difference between morning and evening temperature."[1]

"We recognize a distinct type of continued fever which is not a *compound*. It may be slow or sudden, chiefly slow in its inception, with fever lasting from thirty to seventy days. Temperature rarely above 104°, thirst, anorexia, moist tongue with whitish fur, constipation, some headache, dry skin, often cool extremities, not often delirious, frequent relapses, and rarely terminating fatally after the ninth day. Convalescence slow and often marked by subnormal temperature, 95° to 96°. No post-mortem inspection allowed me."[2]

"We have no typhoid fever in this section, nor have we had since 1870. The prevailing continued fever of the present day (which has been more or less prevalent since 1880) is a type of typhoid fever, certainly. This fever is not malarial, nor is it a combination of typhoid and malarial fever. . . . Many of the symptoms of this fever resemble those of typhoid, but, unlike typhoid, we never have the rose spots, and seldom, if ever, have the low muttering delirium. Neither do we have the ochre-colored discharges; pulse slow in proportion to the temperature; no dulness of hearing."[3]

"I occasionally see cases of continued fever with few or no symptoms except rise of temperature, coated tongue (light brownish fur), accelerated pulse, and a sense of malaise, with slight remissions, about such as are observed in typhoid fever. For want of a better name I call these continued fevers."[4]

"I find in my practice a continued fever, without bowel complications, without hemorrhage, without spots, without much intermission (just about as much intermission as we find in typhoid fever); the fever continues from fourteen days to six weeks; cannot be called either typhoid or malarial. In consultation I find that some of the physicians call it typhoid, some typho-malarial. It amounted to an epidemic here last summer and fall."[5]

[1] Galveston, Texas, W. C. Fisher.
[2] Louisville, Ky., Dr. Thomas J. McKee.
[3] Whitney, Texas, Dr. R. G. Williams.
[4] River View (Cupid), Ky, Dr. B. H. Blair.
[5] Cynthiana, Ky, Dr. Hervey McDonnell.

CONCLUSIONS.—In looking over the whole field, so far considered, the following conclusions may be drawn:

1. The knowledge which we have at the present time of the continued fevers of the United States is in a state of chaos. Individual opinions vary within very wide limits, and no guide, at present, is of practical use in bringing about a greater degree of certainty. The reason for this is largely due to the mild and irregular character of the forms of fever, in regard to which there are the greatest differences of opinion, to their almost invariable termination in recovery, and to their appearance, side by side, with undoubted malarial disease.

2. The widest divergence of views is found in those localities where continued fevers are the least severe, least fatal, and where paludal disease is most prevalent.

The distinct forms of fever recognized by correspondents are:

a. True, typical typhoid fever.

b. True, typical malarial continued fever, bilious or remittent fever.

c. A continued malarial fever of adynamic type. The symptoms which are given as characteristic of this form are said by some to be those of modified typhoid fever, by others of typho-malarial fever.

d. A fever which begins and ends with malarial phenomena, but which presents characteristic symptoms of typhoid fever; not influenced by quinine; called typhoid fever, or typho-malarial fever.

e. A fever which has some of the characteristic symptoms of typhoid fever, but is modified by sudden onset with chill, rapid rise of temperature, the absence of diarrhœa and delirium, general mild character, and favorable termination, called typhoid fever. typho-malarial fever, or thermic fever.

f. A milder form of fever, with none of the characteristic symptoms of typhoid fever—no rose spots, tympanites, diarrhœa, or delirium—of two or three weeks' duration, and with a febrile progress not unlike typhoid fever, with slight elevation of temperature, ending, as a rule, in recovery.

This form is called modified or atypical typhoid fever, typho-malarial fever, and malarial continued fever, bilious or remittent fever, simple continued fever, gastric fever, autumnal fever, fever of locality, as Louisiana fever, catarrhal fever, etc.

Differences of Individual Opinions as to the Classification of Fevers.

Most common classifications of all fevers in localities are:

1. All cases are malarial in nature.
   (Typhoid-like cases are adynamic remittent.)
2. Cases are either:
   a. Typhoid, or
   b. Malarial (remittent or continued malarial).
3. Cases are:
   a. Typhoid (typical).
   b. Malarial.
   c. Typho-malarial (most common).
4. Cases are:
   a. Malarial.
   b. Typho-malarial.
   (No typhoid fever.)
5. Cases are:
   a. Typhoid (typical).
   b. Typhoid (modified, atypical).
   c. Malarial continued, remittent.
   (No typho-malarial).
6. Cases are:
   a. Typhoid.
   b. Malarial continued (remittent).
   c. Thermic fever, or other fever title, as: gastric, catarrhal, simple continued fever.

There is no doubt that the facts of the natural history of our fevers are difficult of interpretation. But the following seem to be conclusions which are reasonable and justifiable:

I. *Typhoid fever* is admitted to be, in its typical form, a widely diffused disease, more nearly approaching a typical character and more commonly met with in this character in the non-malarial region, less typical and less common in the malarial regions, especially in the Gulf Coast Region. But everywhere in this country, or in that portion of it considered in this paper, certain typical symptoms, rose-spots, diarrhœa, and delirium, are less common than in the enteric fever of Europe.

II. The only form of *continued malarial fever* which can be rightfully called by this title, is a fever which has as symptoms: a sudden

onset (preceded by a few days of prodromal malaise), the onset being marked by a chill or chilly sensation, thirst, nausea, vomiting, frontal headache, lumbar pains, a rapid rise of temperature to 104° or over. In six to twelve hours a remission with relief to headache and gastric disturbance, decline of temperature with perspiration. If not promptly treated, such cases may have a recurrence of chills, exacerbations, and remissions with perspiration during five, seven, nine days, to twelve days; the attack ending by crisis. Quinine in these cases has a beneficial and curative effect.

III. *Adynamic malarial fever*, or malarial fever with typhoid symptoms, is so differently described by various observers that its symptomatology is evidently by no means settled in the minds of the physicians of this country. Many describe a typhoid fever, pure and simple, under this name. Adynamic malarial fever has some of the characteristic symptoms of typhoid fever. It is generally believed that diarrhœa, tympanites, intestinal hemorrhage, low muttering delirium, or epistaxis, may all be present, and yet no evidence of the specific typhoid process be found after death. The clinical argument for the malarial nature of these cases is in the recurrence of chills. But what value has this symptom as proof of malarial infection?

To show how our views as to the force of argument derived from experience, and as to the value of symptoms may change, I would refer to those expressed by the medical officers of the United States Army in 1861, in calling many of the cases of fever of that day "bilious remittent," and not typhoid, These were: *a*. The occurrence of the disease in the autumn. *b*. Sudden development of the symptoms. *c*. Epistaxis rare. *d*. Constipation. *e*. Tympanites frequently absent. *f*. Remissions in fever in company with intermissions, generally in the early period. *g*. Tongue furred white or yellow. *h*. Beneficial effect of quinine.

We now know that all of these conditions are frequently met with in cases of unquestioned typhoid fever, and that such distinctions have no weight in the light of observation since 1861. That chills may frequently occur during the progress of typical enteric fever is, I believe, an undoubted fact; and neither in the beginning of a fever, during its progress, nor at the end, can the occurrence of a chill or chills be accepted as evidence of malarial infection. The question is whether these cases are not all, or nearly all, cases of typhoid fever

with variations. The records of cases, in which the diagnosis of remittent fever has been made in the early stages, but later changed to enteric fever, when the fastigium was reached, and the cases in which the diagnosis of remittent fever was held to until death by intestinal hemorrhage or perforation occurred, or until the post-mortem test revealed marked lesions of typhoid fever, are thickly scattered all through hospital reports, the Army and Navy Reports of Great Britain and of the United States, and in the practice of physicians all over this country.

With hardly an exception, there is evidence of a change of views going on throughout the malarial region, in the statement that malarial disease is everywhere diminishing and typhoid fever increasing. And the change results in the loss of a number of cases of malarial fever of this type, and a gain in the number of cases of modified typhoid fever.

IV. *Typho-malarial Fever.*—I will give but a short space to a review of the large amount of conclusive testimony collected on the subject of typho-malarial fever. The amazing variety in the combinations of symptoms supposed to be diagnostic of this form of fever has already been alluded to, and the most conflicting views are held as to what constitutes its symptomatology. It means:

a. A continued malarial fever with typhoid symptoms, which is not influenced by specific (quinine) treatment;

b. Or an enteric fever with intermittent or remittent phenomena in the beginning or the end of its course;

c. Or a continued fever, with gastro-hepatic disturbance;

d. Or a fever of a certain gravity, prolonged, without the specific symptoms of true enteric fever, and not influenced by quinine;

e. Or a fever of a negative and mild character, without the characteristic symptoms of malarial or of typhoid fever.

As at present employed, the term typho-malarial fever has no determined meaning, leads to confusion and to misunderstanding, is a cover for uncertainty and ignorance, and should be discouraged and abandoned. Very many correspondents say that they refuse to recognize the existence of such a disease. The number who believe that typhoid fever is modified by malarial infection is nearly equalled by those who are sure there is no such combination, that the two diseases never unite their symptoms in one individual.

V. There remain to be considered *simple continued fever, gastric fever, autumnal fever*, names used in the non-malarial regions for mild forms of continued fever, without specific symptoms of typhoid or malarial fever. As far as the symptoms of these mild cases are described by correspondents, they are the same as those given in the submalarial and malarial regions, as indicative of remittent fever, bilious fever, and typho-malarial fever. And in view of the admitted fact that the aggregation of symptoms said to belong to simple continued fever and the other forms enumerated, frequently acquire in the later stages the typical symptoms of typhoid fever, or suddenly manifest some grave or fatal complication due to intestinal ulceration, not before suspected, there is every reason to believe that all of these are varieties of enteric fever.

VI. From these considerations the final conclusion may be drawn, that typhoid fever has a much wider distribution in the regions considered in this paper, than is at present believed. Its varieties and modifications are very numerous, and our ideas of its symptomatology must be extended far beyond their present range, in order to include forms now classed erroneously under other names. At any rate, clinical observation alone will not bring a greater degree of certainty, until the pathological anatomy of mild and unclassified forms of fever is more clearly determined, or until the differentiation of their nature, by the study of the microörganisms in the blood, comes to be a matter of practical and general application.

DISCUSSION.

DR. GEORGE M. STERNBERG: The subject is one that has engaged my serious attention at different times, and I quite agree with Dr. Johnston in his conclusions with reference to typho-malarial fever. I think that that term should be abandoned, and from my own study and observation I have been led to think that at least a majority of such cases should be regarded as a mild form of typhoid. In reviewing the army statistics collected during the war, I found, some years since, that typho-malarial fever had a very low mortality, and yet the prevailing idea was that it was typhoid fever with a malarial complication. It is difficult to understand how the complicated disease could have a lower mortality than simple typhoid fever. My own view with regard

to it is, that the line has been drawn between two clinical varieties—mild and severe typhoid.

DR. WILLIAM OSLER: There is no doubt that from the extensive and widespread researches of Laveran we have been able to show, by the characteristic changes in the blood, a decided difference between malaria and typhoid fever. I speak with considerable emphasis on this point, because these observations and researches have been repeated all over the world: in Algiers, in France, in Italy, and in Baltimore, Philadelphia, Boston, and New York in the United States, and recently in India by a most elaborate series of observations by Dr. Carter. The characteristic changes in malaria are as distinctly determined in the blood as are those of tuberculosis of the lungs in the sputa. I desire to impress upon the Association the extreme value of these observations of Laveran. A practical physician can no longer overlook them in discriminating between the different forms of fever.

DR. F. PEYRE PORCHER: It strikes me as very remarkable that Dr. Johnston did not speak once of irritative fever. Many of the cases referred to by his correspondents were probably irritative fever. Dr. Wood, in his treatise, has devoted a whole chapter to its consideration. I think gastric remittent fever is a distinct disease. Irritative fever exists on a mountain plateau, in malarial regions, and in cities.

DR. J. C. WILSON: Dr. Johnston's maps indicating the regional prevalence of typhoid fever fully conform to the views entertained by those physicians who have devoted themselves to the study of fevers in cities. In newly settled countries malarial fevers prevail; after a time typhoid and malarial fevers exist side by side: finally, the malarial fevers wholly disappear and typhoid fever, presenting various modifications of the type, becomes the common, endemic fever. In addition to the test which has been proposed by Dr. Osler, it seems to me there is another which has scarcely received the attention it demands, and that is, the etiological study. The outbreak of a fever of a doubtful type in a village should, in all cases, be subjected to the etiological test, and if there prevail a number of cases the character of which can hardly be definitely recognized, side by side with typical cases of enteric fever, the probability of such cases being modified forms of the typical disease is strong. Confusion in the diagnosis is not only dangerous in regard to the treatment, but, what is of much more consequence, it retards our knowledge of the fevers of this country.

DR. J. E. REEVES: More than a quarter of a century ago I made clinical observations on the subject of enteric fever.[1] I have been following up these observations, feeling more or less interest in the subject, and have gone through several widespread epidemics, some very mild, others malignant and

[1] A Practical Treatise on Enteric Fever; being an analysis of 130 consecutive cases. Philadelphia: J. B. Lippincott & Co., 1859.

exceedingly fatal. I believe typhoid fever is as communicable as any other of our infectious or contagious diseases. In the observations to which I allude I tried to discover and run down the history of typhoid fever in (then) Northwestern Virginia. I discovered that the first case of the disease recognized in all that wide expanse of mountain country could be traced directly to a previous case, which was brought from Athens, Ohio, in 1832. It is the general impression that, since the epidemic cholera of 1832, typhoid fever has become the prevailing type of fever throughout that elevated region of country. I observed this fact, also, that in the low grounds frequently typhoid fever exists with malarial complication, but as we ascend the hill country the pure, unmixed type prevails. I think I have been able to discover the conditions to guide us in separating the two affections, and accordingly to direct the treatment. Whenever I find a broad, flabby, creamy tongue with indentations of the teeth, quinine is not only permissible, but decidedly beneficial. By the use of a few doses I am enabled to eliminate the malarial element, and thus unmask the typhoid form. Some seasons the disease is exceedingly mild, probably there is not one fatal case in twenty. At other times the disease is so exceedingly malignant and fatal that one out of four cases will die. It is a disease peculiar to youth and middle life. I have seen children not more than six months old have pure, uncomplicated typhoid fever. It is rarely seen in the octogenarian. My conclusion is, that typhoid fever may prevail anywhere the infection may be carried. Wherever it finds a lodging-place a centre may be established, from which possibly the disease may spread over the entire neighborhood and assume epidemic proportions.

DR. W. W. JOHNSTON: My object has been to show what the prevailing opinions were in this country in regard to typhoid and other continued fevers. These opinions are based almost altogether on clinical observation, and until a new effort is made by the study of the lesions and microörganisms of fevers of doubtful nature, there is no prospect of a common agreement as to their nature.

THE MANAGEMENT OF THE STAGE OF CONVALESCENCE IN TYPHOID FEVER.

With Remarks as to its Sequelæ.

By JAMES H. HUTCHINSON, M.D.,
PHYSICIAN TO THE PENNSYLVANIA HOSPITAL, SENIOR PHYSICIAN TO THE CHILDREN'S HOSPITAL, PHILADELPHIA, ETC.

Convalescence is a period of repair, and its duration will usually, therefore, be in direct proportion to the length and severity of the illness that has preceded it. A disease, however, may be attended by grave symptoms, but, if not long continued, will be followed by a brief convalescence. This is constantly seen in the ephemeræ of children, in which, after a day or two of hyperpyrexia and intense disturbance of the nervous and digestive systems, the patient may be almost immediately restored to health. So, too, in diseases of longer duration which are unaccompanied by grave organic lesions—as, *e. g.*, typhus fever—the return to health after the subsidence of the active symptoms is generally rapid and uninterrupted. On the other hand, in typhoid fever, which is a disease attended by general impairment of nutrition, marked pathological changes, exhausting discharges, and often intense hyperpyrexia, as well as other severe symptoms, the stage of convalescence cannot be otherwise than long, and, in consequence of the profound debility which is associated with it, a period of great danger to the patient. Indeed, Peter says of it that it is almost a second illness.

The most certain sign of the establishment of convalescence in typhoid fever is the occurrence of a normal temperature on two successive evenings, the morning temperature being at the same time normal, or, as it often happens, subnormal. A fall of temperature does not, however, always indicate that the febrile stage is over. It follows the occurrence of hemorrhage and perforation, and may, therefore, be

the announcement that some grave accident has befallen the patient. It is rare, however, that convalescence in typhoid fever is preceded by an abrupt defervescence, and, if the lysis of the disease does not in all cases possess quite the typical character assigned to it by Wunderlich, it is usually sufficiently distinctive to enable us to recognize it when it occurs. A peculiarity of the temperature is its extreme lability, and hence, very considerable elevations of it may result from causes which, in health, would have no influence upon it.

Accompanying the fall in temperature there is usually a decided diminution in the frequency of the pulse, which, in some cases, does not exceed 35. This association of an infrequent pulse with a subnormal temperature is almost peculiar to the convalescence of typhoid fever, and is generally sufficient, in connection with the appearance of the patient, to enable us to recognize—if we see him for the first time in convalescence—the nature of the disease from which he has suffered. Like the temperature, the pulse under the influence of slight causes often undergoes a marked increase. It is sometimes full, but compressible, at others it is both feeble and frequent, and continues so until complete restoration to health has taken place. It occasionally retains the character of reduplication which it possesses during the febrile stage.

Another characteristic feature of the convalescence of typhoid fever is an appearance of languor and feebleness, together with great pallor of the face. The extremities, on the contrary, often present a slightly cyanotic hue, in consequence of the want of vigor in the circulation. As a result, the patient not infrequently looks more ill at this time than at any other, especially, also, as emaciation becomes now more prominent than before. This pallor is not, however, of long duration, and as the alterations of the muscles and other organs are greater and less readily repaired than those of the blood, the patient may soon present the appearance of greater strength than he really possesses, so that his friends sometimes urge him to undue exertion.

I have not the time, nor is it necessary in this paper to follow out in further detail the symptoms which attend the convalescence of typhoid fever. My object is to suggest a few rules for its proper management, and afterward to refer briefly to some of the least known of the sequelæ of the disease.

With the return of a normal temperature and the subsidence of the

grave symptoms of the febrile stage of typhoid fever even the physician is too prone to overlook the dangers which still threaten the patient, while it is often difficult to persuade the friends of the latter that these exist at all. To a certain extent this incredulity is justified by the difference of opinion which prevails among physicians as to the management of the stage of convalescence. While Collie,[1] on the one hand, cautions us against allowing the patient solid food until he has been free from fever for from ten to fourteen days, and against allowing him to sit up for more than half an hour twice daily during the first week of convalescence, and tells us that the feelings of the patient are dangerous guides at this time; Austin Flint,[2] on the other hand, expresses the opinion that it is a good rule to let the patient have solid food as soon as he himself desires it; and Peabody[3] recommends us to give it immediately after the disappearance of fever. Flint, it is true, in his *Treatise on the Practice of Medicine*,[4] lays down certain rules for the administration of food, which modify very materially the opinion first quoted, as, for instance, that "the patient should be restricted to plain, wholesome well-cooked articles of diet," and that "excess may be avoided by taking food at shorter intervals than in health," and that care with respect to much muscular exertion is important in convalescence from typhoid fever in consequence of a liability to perforation of the intestine;" and Peabody recommends that the meat should be cut up very fine and given in small quantities at a time. A similar, though less decided, divergence of opinion exists among other authors. Thus, Murchison[5] and Wilson[6] restrict their patients to milk, eggs, farinaceous custards, light puddings, beef-tea, chicken-tea, and calf's-foot jelly for seven days after the cessation of pyrexia, and for a longer time if there are signs of intestinal irritation. Watson,[7] Tanner,[8] Wood,[9] and Bristowe[10] give very similar directions. Geissler[11] allows solid food at the end of four or five days. Jaccoud[12] recommends the administration of grilled meats in small

[1] On Fevers, London, 1887. [2] Medical News, April 3, 1884. [3] Ibid.
[4] A Treatise on the Principles and Practice of Medicine, Philadelphia, 1868.
[5] A Treatise on the Continued Fevers of Great Britain, London, 1887.
[6] A Treatise on the Continued Fevers, New York, 1881.
[7] Lectures on the Principles and Practice of Physic, Philadelphia, 1872.
[8] The Practice of Medicine, Philadelphia, 1866.
[9] A Treatise on the Practice of Medicine, Philadelphia, 1855.
[10] A Treatise on the Theory and Practice of Medicine, Philadelphia, 1879.
[11] Schmidt's Jahrbücher, Bd. cciv., 1887. [12] Traité de Pathologie Interne, Paris, 1879.

quantities when the febrile cycle is terminated, although he admits that it gives rise to a recrudescence of fever, which he terms *febris carnis;* and Nothnagel[1] gives solid food as soon as two or three days after the disappearance of fever, sometimes even allowing meat before this has taken place in cases in which the patients are very low; and Cayley[2] does very much the same thing, founding his practice upon the fact that a boy recovered whose mother removed him from under Dr. Cayley's care rather than allow him to be, as she thought, starved.

My own experience has been in accord, decidedly, with those who have found a too early return to solid food prejudicial to the speedy recovery of the patient, and I have long since come to agree with Sir Thomas Watson that the management of the convalescence of typhoid fever is scarcely of less importance than that of the fever itself. It is certainly not less difficult. A part of this difficulty arises from the impossibility of convincing the patient's friends that his voracious appetite cannot be fully indulged without dangers. My own custom has been to continue the administration of milk, which forms the almost exclusive diet of my patients in the febrile stage, in cases in which it is well borne—and there are few in which it is not—for three or four days after the occurrence of normal evening temperatures, with the addition of animal broths.[3] At the end of this time I give eggs (soft-boiled), the juice of rare meat, milk toast, and other farinaceous articles of food. At the end of a week the soft part of oysters and fish are added to this dietary, and at the end of ten days the light meat of broiled chicken, and at the end of two weeks butcher's meat. All these articles of food are given in small quantities at a time, but may be repeated at first once or twice, and later oftener during the day. If milk be given in sufficient quantity, say six ounces every two hours, it will fully meet the wants of the system. The only objections that can possibly be urged against this plan of treatment are, that the continued use of milk is tiresome to the patient and that it produces constipation. The first objection is of little moment; if there are dangers attending the administration of other kinds of food, and

[1] Clinical Lecture on the Treatment of Typhoid Fever, Medical Press and Circular, March 9, 1887.

[2] On Some Points in the Pathology and Treatment of Typhoid Fever, London, 1880.

[3] In cases in which the patient positively refuses to take milk or in which it is rejected by the stomach, Murdock's liquid food, or Reed & Carnrick's beef peptonoids may often be given with advantage.

may be obviated by giving, after the first few days at the same time with the milk, some of the farinaceous articles of food, which will make it more acceptable to him, and will at the same time prevent, to a certain extent, its tendency to produce constipation.

In order to show that the opinions I hold on this point are not the result of theory, but the outgrowth of experience, and that I am not so prejudiced in favor of my own views as to be unwilling to put those of others to the test of trial, I will briefly refer to a few cases which were under my care during my recent term of service at the Pennsylvania Hospital. In one of them, a Swede, already several days advanced in convalescence, in the absence of the nurse took some meat and potatoes from the plate of his immediate neighbor in the ward, and, although the amount actually eaten was believed to be small, a rise of temperature of nearly six degrees was observed in the course of a few hours. In many others of my cases in which similar recrudescences occurred, which were otherwise inexplicable, it was often found upon interrogating the patients or their neighbors, that the former had partaken of improper food, brought to them in mistaken kindness by their friends. During last summer, also, in view of the fact that my views might be criticised on the ground that my experience had been one-sided, I ordered several of my patients to have an increase of diet, such as eggs, the farinaceæ, and meat in small quantities almost immediately after the cessation of pyrexia, but only in cases in which the other symptoms were favorable. In every case a recrudescence of fever was noted, and in one a short relapse seemed to have been induced by the change of diet. Without going so far as to assert that a relapse may be caused by a too early return to solid food, I will say, that this is not the only case which has come under my observation in which it is difficult to find another explanation. It is only proper to add that in some cases in which liquid food is persistently vomited, solid food will often, as was pointed out by Trousseau, be perfectly well borne and must frequently be given, even during the febrile stage, in order to save life. Peter has called attention to a condition occurring occasionally in convalescence in which there are persistent anorexia, epigastric distress, and tympanitic distention of the stomach. In this condition, he says that the patient is to be nourished with extreme caution and that it demands greater care than the more usual one in which the appetite is imperious.

Closely allied to the subject of the regulation of the diet is that of the administration of alcohol. As I do not think it necessary to give stimulants in all cases of typhoid fever during the febrile stage, so I believe there are many in which they may be withheld with perfect propriety during that of convalescence. On the other hand, it occasionally happens that they then become for the first time necessary. There is no doubt, moreover, that recovery is often hastened by their judicious use. I should, therefore, be influenced by the same reasons in giving them during convalescence as have guided me at an earlier stage of the disease, to wit, the condition of the pulse and heart, the degree of general prostration, and the presence or absence of ataxic symptoms. In a moral point of view, it is better not to give them, if it can be avoided, to very young persons who have been previously unaccustomed to their use, and in all cases, if possible, to discontinue them before the patient passes from under the care of the physician. Port and sherry may often be substituted for the stronger alcoholic liquors at this time with advantage, and so may the lighter wines. The malt liquors will often be acceptable to the patient and may be resorted to in cases in which they are not contraindicated. Huxham's tincture has always seemed to me a useful tonic at this time. Other tonics may also be prescribed with advantage. If the patient is anæmic, one of the milder preparations of iron, the lactate, for instance, will often expedite the recovery. In cases in which the digestion is sluggish, tincture of gentian before the principal meals and pepsin with muriatic acid have often proved of service in my hands.

In severe cases of typhoid fever, the patient should maintain the recumbent position for at least a week after the beginning of convalescence. He should not be allowed to leave his bed even to pass his water or to have a movement of the bowels, and in cases in which the prostration is extreme, not even to sit up in bed or to feed himself. That these rules are not too stringent is shown by the buzzing in the ears, the dizziness, the vertigo, faintness, and even fatal syncope which have followed the slightest voluntary movement, and by the staggering and uncertain gait when the patient attempts to walk.[1]

[1] While unquestionably among the most frequent causes of recrudescences of fever is getting out of bed too soon, it is rather a singular fact, pointed out by Da Costa, that remaining in bed too long will frequently cause a febrile condition which will generally disappear if the patient is directed to get up.

At the end of a week he may be allowed to sit up for half an hour, and the next day for a longer time, or the act may be repeated, in this way being accustomed and encouraged to make a little more exertion each day. Usually, however anxious he may have been for the privilege, he is, at first, willing enough to return to his bed at the end of the prescribed time, having learned, by experience, how little strength he really possesses. As soon as the patient's strength will admit of it he should be removed to an adjacent room. This often has a very good effect, for not only does it permit his own room to be thoroughly aired and cleansed, but the change is often agreeable to him, and seems, as it is, a decided step forward in the progress of his recovery. In very mild cases it will, of course, not be necessary to keep the patient quite so long a time in bed; in fact, it will often be impossible to do so. Exercise in the open air is to be encouraged as soon as the patient's strength will permit his going up and down stairs, and even before this if he can be carried. Driving is also useful when the patient is sufficiently strong to enjoy it.

The patient is more emotional in convalescence, and is much more readily moved to tears than in health. He should, therefore, be carefully guarded against all exciting or depressing influences. I have known a severe relapse apparently induced on the eighth day of convalescence by the communication to the patient by her husband of the news of the death of their child. A slight rise of temperature was noted the same evening; this was followed by a daily increasing temperature, until 106° F. was reached on the seventh day of the relapse, by epistaxis, tympany, rose-colored spots, sudamina, and, in fact, by all the symptoms of a well-marked attack of typhoid fever.

The patient should not be allowed to have too many visitors at this time, and only those should be permitted to see him who are possessed of sufficient judgment not to excite and fatigue him by referring to business matters or to topics which are disagreeable to him. After visiting days at the Pennsylvania Hospital, it is not infrequently found that the patients who have had visitors present evidences of recrudescences of fever of more or less gravity. On the other hand, the influence for good of a bright, cheerful, and judicious friend cannot be overestimated. Insomnia is sometimes a troublesome symptom in convalescence. I recollect that when suffering from it mysef, during my recovery, several years ago, from typhoid fever, it yielded to a glass

of ale taken just before settling for the night. Sometimes, especially when the insomnia is associated with nervous restlessness, small doses of camphor and opium, repeated if necessary, are useful. I should not be inclined, for obvious reasons, to have recourse to the bromides in this condition. Although I have had no experience with urethan, I should be disposed to resort to it in case of failure with the remedies above referred to.

Occasionally recovery is retarded by the occurrence of diarrhœa during convalescence. This may be due either to an unhealed condition of Peyer's patches, to an intercurrent attack of inflammation, or to the irritation set up in the intestines by improper food or some other cause. If dependent upon the last-named causes, the diet should be regulated and a little bismuth and opium given. If there is reason to believe that it is produced by a sluggish condition of the intestinal ulcers, nitrate of silver will often be found an admirable remedy, giving at this time, in my experience, better results than during the febrile stage of the disease. Oil of turpentine is often also useful in this condition. When there is reason to believe that inflammation exists, acetate of lead or nitrate of silver may be given with advantage.

Sometimes, instead of diarrhœa, the patient suffers from constipation, an accumulation of feces even taking place in cases in which free bowel movements apparently occur daily. It is often the source of much distress to the patient and may occasionally be the cause of continued elevation of the temperature. If it does not yield to enemata, mild laxatives may be given by the mouth. The late Dr. W. W. Gerhard was accustomed to prescribe in this condition a tablespoonful of olive oil, and often with the happiest results. If this fails, castor oil, and if this is also unsuccessful, some one of the least active of the purgatives may be given; but recourse should never be had to the drastic cathartics, as their use has been followed, even at this stage, by perforation. Relief may also often be afforded by adding stewed fruit or some other laxative article to the diet, if convalescence be sufficiently far advanced to permit it.

During convalescence the patient frequently complains of chilliness, and requires an increased amount of clothing. He is more apt to suffer from exposure to cold than during the febrile stage. In fact, thoracic inflammations are not uncommon at this stage as complications.

Among the most frequent interruptions in the favorable progress of

convalescence are "recrudescences of fever." I have incidentally alluded, in the course of my paper, to some of the causes of these. They are usually slight, and in many cases escape detection. With little or no warning the temperature will be found to have risen several degrees. Thus, in a case of which I have notes, a severe toothache was the only cause which could be discovered of a sudden rise from 98° to 104° F. In another case, in which a patient was allowed to sit up too long on the eighth day of convalescence, a similar rise was noted, and I have known the same thing to occur at about the same relative time in a patient who had been allowed to read in bed. It will also take place after the communication of exciting or depressing news, or the ingestion of improper food. In fact, the temperature is, at this stage of the disease, in so labile a condition, that the slightest cause will produce this effect. These recrudescences are, however, simply attacks of irritative fever, and, unlike relapses, have no specific character. They rarely last for more than thirty-six hours, and usually subside upon the removal of the causes which produced them, although, in a few instances, the continuance of the fever requires special treatment.

I have not time, unfortunately, to refer very fully in this paper to the subject of relapses. Notwithstanding their frequency and the care with which they have been studied, no satisfactory explanation for their occurrence has as yet been advanced. While they are unquestionably met with oftener in some epidemics than in others, they have apparently followed as effects the same causes as I have shown may produce a simple recrudescence, such as errors of diet, bodily and mental fatigue, the exciting and depressing emotions, and so on. We can understand readily enough how the causes above alluded to may give rise to attacks of irritative fever, but that they should cause a return of a specific disease is less intelligible. Moreover, relapses have occasionally occurred in cases in which the patients were carefully and successfully guarded against all disturbing influences. They can, therefore, be regarded only as at most exciting and not as essential causes.

It has been maintained by some authors that a relapse indicates that a new infection has taken place; but this hypothesis, even if we admit that it accounts for those cases in which the patient is allowed to remain in the place in which he acquired the disease, does not ex-

plain those in which he has been removed during the first attack to a hospital, where all the sanitary arrangements are presumably perfect. Griesinger has endeavored to explain the occurrence of relapses by suggesting that they may possibly be due to a fresh contagion from other patients with typhoid fever in the same ward, but this explanation is rendered improbable by the fact that relapses have taken place even in cases which were thoroughly isolated. To adopt Griesinger's explanation, it would be necessary to assume that a patient just recovered from an attack of the disease is more susceptible to the action of the contagion than patients suffering from other diseases, which seems improbable, to say the least. It has also been maintained that relapses are caused by the inoculation of the previously healthy Peyer's patches by the typhoid poison which is thrown off with the sloughs from those first affected. Maclagan alleges that relapses are most frequently met with in cases in which constipation is present in the primary attack—a condition which he regards as favorable to absorption; but this is opposed to the experience of almost every one who has paid any attention to the subject. In the cases which have come under my own observation it certainly was not the case, diarrhœa having been present in the large majority of them. It is more likely, as suggested by Liebermeister, that part of the poison remains latent somewhere in the body, not developed, destroyed, or expelled during the first attack, but brought into activity by an exciting cause.

Different plans of treatment have at various times, but on insufficient grounds, been charged with increasing the predisposition to relapse. Relapses occur in patients of both sexes and of all ages with about the same frequency. Sometimes more than one relapse is observed in the same patient. I have reported a case[1] in which three well-marked relapses occurred, and a similar case is reported by Anders.[2]

But relapses and recrudescences of fever are not the only accidents which may happen in the course of the convalescence of typhoid fever, and all danger from other causes to the patient's life and health is by no means past when a normal evening temperature has been observed for several evenings in succession. Complications and sequelæ frequently occur at this stage of the disease, which may, on the one hand, be so

[1] American System of Medicine, vol. 1, p. 303, Philadelphia, 1885.
[2] Med. and Surg. Reporter, Phila., July, 1882, xlvii. p. 66.

slight as to cause only a transitory inconvenience, or, on the other, if not immediately fatal, seriously impair his health or mental faculties for the rest of his life. Several years ago Dr. W. W. Keen delivered a Toner lecture in which the "surgical complications and sequels of the continued fevers"[1] are very fully considered, and I propose now to devote a few minutes to consideration of the complications and sequelæ which more especially interest the physician as distinguished from the surgeon.

For the sake of convenience, they will be discussed under the following heads:

Sequelæ affecting the nervous system.
Sequelæ affecting the circulatory system.
Sequelæ affecting the respiratory system.
Sequelæ affecting the digestive system.
Sequelæ affecting the genito-urinary system.
Sequelæ affecting the skin, etc.

Sequelæ affecting the Nervous System.

The sequelæ which may be included under the first head are neither few nor unimportant, consisting, in fact, of almost every variety of nervous disease. Some of them occur within a few days of the establishment of convalescence, others not until a much longer time has elapsed—in one instance not until after six months had passed.

Disturbances of speech amounting either to complete aphasia or only to embarrassment of that faculty are reported or referred to by Hutinel,[2] Ebstein.[3] Jaccoud,[4] Gubler,[5] and the author of the article on typhoid fever in the *Nouveau Dictionnaire de Médecine et de Chirurgie*.[6] Hutinel says that aphasia always occurs in children and more frequently in boys than in girls (10 out of 12). It has also occurred during the fever and been prolonged into the convalescence. In some cases it is unquestionably due to embolism of a cerebral artery, but in others the rapidity with which recovery takes place shows that it must have depended upon another cause, a view, the correctness of which is proved by the fact that no embolism has been found in some of the

[1] Smithsonian Institution, Washington, 1877.
[2] Etude sur la Convalescence et les Rechutes de la Fièvre Typhoïde, Paris, 1883.
[3] Quoted by Nothnagel, Deutsches Archiv für klinische Medicin, 9, 1872.
[4] Loc. cit.
[5] Archives Générales de Médecine, xv., 5 series.
[6] Homolle: vol. xxxvi., Paris, 1884.

cases in which autopsies have been obtained. Leyden[1] believes that in such cases the aphasia may have been due to slight encephalitis in which the exudations are readily absorbed. Anæsthesia of various parts of the body has been observed by Gubler[2] and Nothnagel,[3] and in a case reported by Dr. James J. Putnam[4] there was also analgesia. In this case, which is of some interest, the patient, some time after apparent cure, noticed that some small wounds on the hands did not cicatrize readily and that certain wounds on his hands and arms did not heal. This analgesia was present two years later when the patient first came under Dr. Putnam's observation and extended to the skin of the left arm and shoulder and left side of neck and trunk, extending downward as far as the eighth rib. Light touches were perceived, but the prick of a pin and water at 43° C. were not felt; there was also diminution of sensibility to electric currents. In this case a gradual improvement took place as the result of treatment.

Eight cases of contraction of the extremities are reported by M. Aran,[5] and a case of contraction of the hands by Gubler,[6] and the possibility of this complication is referred to in the *Nouveau Dictionnaire de Médecine et de Chirurgie*, and by Dewèvre. Nothnagel refers to a case reported by Paulicki, in which there were tonic contractions of the interosseous muscles, lasting from a quarter to half an hour; similar contractions of the plantar muscles being induced when the patient supported himself upon his toes. Convulsions may also take place in this as well as in the febrile stage of the disease. Amaurosis, with other eye-symptoms, has been observed by Gubler, and strabismus by Nothnagel, and atrophy of the optic nerve by Teale and Mooren.[7] Cases of acute general paralysis are reported by the same authors, and cases of hemiplegia by Hutinel, Nothnagel, and Beau. Nothnagel believes that paraplegia may also occur. Paralysis of the vocal cords has been reported by Türck[8] and Nothnagel, and cases of paralysis of the median nerve, of the perineal nerve, of the left ulnar, of other spinal nerves, of some of the cerebral nerves, of the serratus magnus, and of the left upper extremity by Nothnagel, and of the velum palati and facial nerve by Gubler.[9] Cases of chorea

[1] Quoted by Hutinel.
[2] Archives Générales de Médecine, vol. xv., 5 series.
[3] Deutsches Archiv für klinische Medicin, 9, 1872.
[4] Transactions of American Neurological Association.
[5] Quoted by Rathery.
[6] Loc. cit.
[7] Quoted by Nothnagel, loc. cit.
[8] Quoted by Nothnagel.
[9] Loc. cit.

are reported by Rilliet and Barthez. Infantile paralysis and paralysis agitans have also been observed after typhoid fever, and so have locomotor ataxia and paralysis of the bladder.

Tremor has also been observed by Rathery.[1] In one case it was present fifteen months after the attack of fever. This symptom is also referred to by Hutinel. Carl Samuel Freund,[2] in his inaugural dissertation *On Inflammation of the Brain in the Convalescence of Typhoid Fever*, relates several cases, and a case of abscess of the brain, which occurred during a relapse, is reported by Max Weiss.[3] Raymond[4] reports two cases of ascending myelitis observed during the convalescence of typhoid fever. In one case the paralysis occurred sixty days after the beginning of the attack of fever, was fifty days in reaching its acme, remained stationary for five days, and was thirty-four days in disappearing. In the second, the paralysis was first noticed thirty-nine days after the beginning of the disease, was twenty-eight days in reaching its greatest development, was stationary six days, and thirty-three days in disappearing. In these cases at the end of a certain time the lower extremities were the seat of ichthyosis and of œdema, principally seated at the level of the tibio-tarsal articulation. Gubler also refers to cases of myelitis. Inflammation of several of the nerves has been observed by various authors.

Trophic disturbances, apart from the loss of the hair and changes in the nails, are rare after an attack of typhoid fever.

A certain amount of enfeeblement of the mind is admittedly present in a large number of cases during convalescence. In this condition there may be impairment of memory, slowness of perception, and difficulty in ideation. In one case, for instance, a young lady amused herself with a doll, and in another a college student forgot his Latin. This condition would appear to be most commonly met with in patients who have not been allowed sufficient nourishment during the febrile stage of the disease, and will often disappear under the influence of a more generous diet. On the other hand, hallucinations may sometimes be present, as in a case reported by Herbet,[5] in which the patient believed for ten days that his nose had been cut off by a horse

[1] Des Accidens de la Convalescence, Paris, 1875.

[2] Breslau, 1885.

[3] Wiener med. Wochensch., vol. xxxii. No. 50

[4] Revue de Médecine, v., 1885.

[5] Considerations sur les Troubles de l'Intelligence qu'on Observe dans la Fièvre Typhoïde, Archives Générales de Médecine, 5e. s., t. vi. p. 737

and stuck on again by a surgeon. In another case, reported by the same author, the patient was under the delusion that he had been promised a situation, and on his discharge from the hospital, perfectly cured in other respects, asked for a letter of recommendation. Cases of more active delirium have been recorded by several writers: Homolle, Hutinel, and Baumler,[1] among others; the last-named author calls special attention to the frequency with which delirium occurs at the time of the subsidence of the fever, or of the beginning of convalescence, and says that its character reminds one of that seen in delirium tremens. Herbet refers to a case of delire des grandeurs, and so does Hutinel.

Sequelæ affecting the Circulatory System.

The peculiar degeneration of the cardiac muscles which has been fully described by Hayem[2] and others, persists, of course, through the early part of convalescence, and is unquestionably the cause, in some instances, of the sudden deaths which occasionally take place at this stage of the disease, as, for instance, in the cases reported by Louis, Bailly, and Jaccoud; but that it cannot be the sole cause of this accident is shown by the fact that sudden death has occurred in cases in which no trace of this degeneration could be detected at the autopsy, as in two cases reported by Déjerine.[3] Still, it would seem to be, by far, its more frequent cause, since, in a series of forty-eight cases analyzed by Dewèvre,[4] the heart was found to be softened, pale, and of the dead leaf color in 15; in 16 it had undergone granular or fatty degeneration; in 3 there was poliferating endarteritis of the small vessels; in 8 the right ventricle contained a fibrinous clot, apparently formed during life, and in 8 others a cardiac thrombosis, accompanied generally by thrombosis of the cavæ and pulmonary veins, was found.

While sudden death sometimes also occurs in the febrile stage, it occurs with even greater frequency during that of convalescence. This possibility was pointed out long ago by Graves, who says that even if the fever has declined, even if a favorable crisis has taken place, one is not justified in banishing all anxiety or in relaxing vigilance, because even then the patient may die in a few minutes. This mode

[1] Deutsches Archiv für klinische Medicin, 1867, iii. 532.

[2] Leçons Clinique sur les Manifestations Cardialques et la Fièvre Typhoïde, Paris, 1875.

[3] Comptes Rendus, Soc. de Biologie, 1885, p. 769.

[4] De la Mort Subite dans les Fièvre Typhoïde, Archives Générales de Médecine, 1887, ii. p. 385.

of termination of the disease is said by Dewèvre to occur most frequently in patients between the ages of 22 and 25 years; old people and infants apparently enjoying an immunity from it. It is more frequent in men than in women (114 to 26), and by no means occurs oftenest after the severest forms of the disease, Dewèvre asserting that it is most common after *la forme moyenne*. The exciting cause may be very slight, such as merely extending the hand, or the emotion induced by seeing a relative, but it has also occurred in cases in which no exciting cause could be discovered, and several patients have been found dead in bed in an attitude that showed that they were stricken in their sleep. It may sometimes, however, be due to embolism of a cerebral artery.

Death is not always quite so sudden, and it may then be preceded sometimes by dyspnœa, which auscultation does not explain, sometimes by bronchitis or congestion of the lungs, sometimes by buzzing in the ears, by irregularity and intermittence of the pulse, by *bruit de souffle*, and more rarely by syncope and convulsions. Dieulafoy,[1] who has given much study to the subject of sudden death in this disease, is inclined to attribute it, in the absence of disease of the heart, to a reflex irritation, which starting from the intestine extends to the mesocephalon. If the pneumogastric is the only centrifugal nerve affected, there will be syncope, which may be at once fatal; if the respiratory nerves are also affected, the respiration will be arrested coincidently with the heart. In the majority of the cases, he says, the cellules of the facial nerves or of the nerves of the limb are at the same time irritated, a fact which explains the convulsions which so frequently usher in death.

Embolism of the pulmonary artery, originating in most cases from thrombosis of the femoral veins or from that of the cerebral veins, may also cause death more or less rapidly.

Embolism of the other arteries of the body may also occasionally occur and give rise to gangrene, as in the case of those of the extremities. Where, however, gangrene occurs in the absence of the signs of disease of the heart, the probability is that it is due to a form of arteritis, which will be described further on in this paper.

Pericarditis occurred in one of my patients, who, after passing

[1] De la Mort Subite dans la Fièvre Typhoïde, Paris, 1869.

through an attack of fever of moderate severity, was attacked by erysipelas on the thirteenth day of convalescence and subsequently by pleurisy and still later by pericarditis. So many cases of thrombosis of the different veins of the body occurring in the course of typhoid fever have been reported that it does not seem necessary to dwell upon the subject in this paper further than to call attention to the extreme frequency with which this complication occurs during the stage of convalescence.

Dr. E. Barié[1] has recently called attention to inflammation of the arteries as a sequel of typhoid fever. This inflammation affects both the larger trunks and the minute arterioles. The arteries of the lower extremities in the case of the large trunks are most frequently affected (in 22 cases out of 24 cited by the author). This inflammation usually occurs at about the time the patient begins to leave his bed and to move about—a fact which explains its frequency in the lower extremities—and occurs just as often after mild as after severe attacks. Dr. Barié distinguishes two varieties: 1. Acute obliterating arteritis. 2. Acute parietal arteritis. The first variety is constituted anatomically by an embryonal infiltration of the three coats, disappearance of the smooth condition of the intima, which becomes uneven and granular. This has, as a consequence, the production of a secondary thrombosis, which in course of time becomes a dense gray mass adherent to the parietes of the artery. Very often the inflammation of the artery is accompanied by a certain amount of periarteritis. If the lumen of the affected artery is completely obliterated and the collateral circulation is not quickly established, mortification ensues and the limb assumes the appearance of dry gangrene. In exceptional cases, in consequence of the simultaneous occurrence of venous thrombosis or of phlebitis, moist gangrene may follow the mummifying variety, or substitute itself for it.

The principal symptoms of obliterating arteritis are as follows: Acute pain occurring more or less suddenly and seated in the course of the affected artery, sometimes localized in a restricted region, as, for instance, the thigh, calf, or Scarpa's triangle, sometimes occupying the whole length of the limb, and increased by pressure, upon assuming the erect position, and by the movements of walking; diminution of

[1] Contribution à l'Histoire de l'Arterite Aigue Consecutive á la Fièvre Typhoïde, Revue de Médecine, iv., 1884.

the fulness and finally suppression of the pulsations of the artery; swelling of the limb without œdema or redness; bluish mottling of the skin; sometimes, although rarely, purpura; diminution of the temperature of the limb with or without disturbances of sensibility, such as formication and partial anæsthesia, and finally the occurrence in the course of the artery of a hard and tender cord.

The parietal arteritis is only a variety of the preceding and has consequently the same symptoms, but in a less degree of development, except, of course, that the hard, painful cord is absent. It is said, however, that the diminution of the pulsations of the artery is occasionally preceded by an exaggeration of their amplitude and that in a few cases the temperature of the affected limb has been observed to be higher than that of the other.

It must be borne in mind that some of the symptoms of the obliterating variety may arise from an embolus, but the presence of a valvular murmur and of other signs of disease of the heart, and the suddenness of the seizure, will enable us to recognize without difficulty the cases dependent upon this cause.

The therapeutic indications in the milder forms are best fulfilled by rest in bed, the application of emollients or soothing ointments to the limb, and wrapping it in cotton. In cases in which gangrene has occurred the patient should be supported by tonics and a liberal diet, and appropriate antiseptic dressing should be applied to the part. This endarteritis may affect the arteries of the heart; a condition which is believed by Landouzy and Siredey[1] to be occasionally the cause of sudden death.

Sequelæ affecting the Respiratory System.

The complications and sequelæ connected with the respiratory system are less frequent and important than those which we have just considered. They would seem also to be rather the result of accidental causes than of any inherent tendency in the disease to their production. Pleurisy occurred in four cases which have come under my observation—in one of them with a fatal result—all of which were well advanced in convalescence. Gairdner and Liebermeister report similar cases. Pneumonia also occurs. In regard to the influence of typhoid fever in the production of phthisis different opinions are held.

[1] Contribution à l'Histoire de l'Artérite Typhoïdique, Revue de Médecine, v., 1885.

Jaccoud believes that an attack of typhoid fever precipitates the course of tuberculosis, if it already exists, but he knows of no case in which it has provoked the formation of tubercles. On the other hand, he says, it may give rise to caseification of the lung and to phthisis in consequence of the persistence of lobular foyers, exactly as it does in measles. Flint reports the case of a man who left the hospital on the thirty-third day after his admission and resumed the occupation of a laborer, who eighteen days subsequently was readmitted with cough and fever, and in whom tuberculosis was finally developed. In my own experience I have known cases of typhoid fever to terminate in acute phthisis, and should carefully guard any patient having a tendency to this disease from all causes of inflammation of the lungs. On the other hand, I have had cases under my care which I at first believed to be typhoid fever, but which a further study showed to be acute phthisis.

Inflammation of the larynx and laryngeal ulcers also occur in the stage of convalescence, but this subject has been so fully discussed by Dr. Keen in the lecture alluded to above, that it is only necessary here to make this brief reference to it.

Sequelæ affecting the Digestive System.

Accidents connected with the digestive system may also occur at this time. There is not only a possibility of the ulcers of Peyer's patches remaining unhealed and thus giving rise to perforation of the intestine as late as the one hundred and tenth day,[1] and even after the patient has been discharged from the hospital apparently well, but peritonitis from propagation of irritation from the intestine to the sereus membrane has also been observed at this time. Such cases are referred to by Hutinel, who also alludes to typhlitis and perityphlitic abscesses as complications of this stage of the disease.

Jaccoud describes an ulcerative colitis which is sometimes set up in convalescence and which is accompanied by fever, sometimes mistaken for that of a relapse. It usually gives rise to a saguinolent diarrhœa and sharp intestinal pains which will generally enable us to recognize it. It has occurred, as autopsies have shown, after the cicatrization of the typhoid ulcers and is especially intense in the region of the sigmoid flexure. It is not invariably fatal.

[1] Morin, Des Perforations Intestinales dans le Cours de la Fièvre Typhoïde. Paris, 1869.

Other complications coming under this head are intestinal hemorrhage, icterus, and ulceration and gangrene of the mouth. In a case which Franklin[1] reports gangrene began in the upper gum and caused in five days necrosis of the superior maxillary bone. An interesting case of contraction of the intestines is recorded by Young,[2] in which, after death, the lower twenty-five inches of the ileum were found so greatly contracted that the first joint of the thumb could be introduced only with great difficulty into the widest part of it. At about two inches above the ileo-cæcal valve, and again near the upper part of the contraction was a firm band of fibrous tissue. Through these constrictions only the tip of the little finger could be passed. Above the constriction for about fifteen inches the small intestine was enormously dilated, so as to resemble the stomach. The immediate cause of death in this case was a fall from a horse.

Sequelæ affecting the Genito-urinary System.

The genito-urinary system furnishes also a certain number of sequelæ to typhoid fever, such as albuminuria, which Jaccoud says is rare, and catarrh of the bladder, which is not so infrequent, especially in those cases in which this organ has been allowed to become over-distended during the febrile stage.

Velpeau appears to have been the first to call attention to orchitis[3] as an occasional complication or sequel of typhoid fever. Although it may occur in the febrile stage, it is much more frequently met with in convalescence (20 times out of 27). Since Velpeau, Chevedergne,[4] Bouchut,,[5] Hanot,[6] Vallin,[7] Widal,[8] Duffey,[9] Liebermeister,[10] Eloy,[11] Larquier,[12] Sadrain,[13] Hutinel,[14] Ollivier,[15] Hamilton,[16] Manly,[17] and Sabourin,[18] all refer to it.

[1] Quoted by Hutinel.
[2] Medical Press and Circular, 1886, xlvi. p 471.
[3] Orchitis, although a surgical affection, is considered here because it has escaped the notice of Dr. Keen.
[4] De la Fièvre Typhoïde et les Manifestations, Thèse, Paris, 1864.
[5] Quoted by Ollivier.
[6] Orchite dans la Fièvre Typhoïde, Arch. Gén. de Médecine, 1878, t. ii. p. 595.
[7] Quoted by Ollivier.
[8] Fièvres Typhoïdes Compliqués d'Orchite, Bull. de la Soc. Clinique de Paris, 1877, t. i. p. 142.
[9] Dublin Journal of Medical Science, vol. liii. p. 97.
[10] Ziemssen's Handbuch.
[11] Union Méd , 1883.
[12] De l'Orchite dans la Fièvre Typhoïde, Paris, 1882.
[13] De l'Orchite dans la Fièvre Typhoïde, Thèse de Doctorat, Paris, 1882.
[14] Etude sur la Convalescence et les Rechutés de la Fièvre Typhoïde, Paris, 1883.
[15] Contribution à l'Histoire de l'Orchite Typhoïde, Revue de Médecine, 1883, iii.
[16] The Lancet, 1882, vol ii. p. 1039.
[17] The Lancet, 1882, vol. ii p. 1065.
[18] Orchite Parenchymateusè dans la Convalescence de la Fièvre Typhoïde, Bull. Soc. Clinique de Paris, 1878, t. ii.

In 27 cases collected by Ollivier, the age was 4 months in 1, between 16 and 30 years in 14, between 30 and 35 in 3, and over 40 in 2. The age is not given in 7, but as in 6 of these the patients were soldiers, it is fair to assume that they were young. The degree of severity of the attack of fever which has preceded it appears to exercise an influence upon the frequency of this complication. In 4 cases the fever is said to have been slight, in 13 of medium severity, in 6 grave, and in 4 adynamic. When this complication occurs in convalescence, the fever to which it gives rise is frequently mistaken for that of relapse. The testicle is more frequently affected than the epididymis. In 13 cases both were involved; in 9 the testicle alone, and in 4 the epididymis only. There is usually at the same time engorgement of and pain in the spermatic cord; in 15 cases the right side was affected, in 11 the left, and in 1 the side is not mentioned.

The average duration was twelve days, but cases in which suppuration occurred lasted much longer, in one case as long as two and a half months. All the patients recovered, except the infant of four months. In 8 cases, however, induration persisted, and in 1 atrophy, and in 6 suppuration occurred, the microscope showing in the discharge of some of these the débris of the testicle. The orchitis following typhoid fever differs from that caused by gonorrhœa presenting more resemblance to that following mumps, except that it does not nearly so often give rise to atrophy. It may sometimes be of rheumatic origin. Widal attributes it to thrombosis of the spermatic vein, and Bucquoy to masturbation, which he says is not infrequently practised in the convalescence of typhoid fever, when the genital functions are said to be very active.

Before concluding this paper I will refer briefly to a case in which diabetes appeared to be a sequel of typhoid fever. The patient was a lady, about forty years of age, who has been for some years under my professional care. Three years ago she had a severe attack of typhoid fever, during which her urine was examined and found to be normal. Her convalescence was tedious and protracted by a succession of boils, which led to the reëxamination of the urine and to the detection of sugar in it. The saccharine condition of the urine has continued up to the present time, although she is able, by carefully regulating her diet, to maintain a fair degree of health. It will be of interest to

add that she is hereditarily predisposed to diabetes, her mother having died of this disease at an advanced age.

Sequelæ affecting the Skin, etc.

Boils and subcutaneous abscesses are not infrequently met with in the convalescence of typhoid fever, and although usually not the cause of any serious trouble, occasionally wear out the patient, especially when they coexist with visceral productions of the same character. Rathery, Jaccoud, Dyce-Duckworth, and Liebermeister have all reported cases of this kind. On the other hand, some of the cutaneous eruptions are said to be favorably modified by an attack of typhoid fever.

Although erysipelas is strictly not a disease of the skin, it will be more conveniently considered under this head. It appears to be quite a frequent, and, unfortunately, a grave complication of the convalescence of typhoid fever. I have already referred to one case in my own practice, in which a severe attack of erysipelas occurred on the thirteenth day of convalescence, and was itself succeeded by pleurisy and pericarditis; and cases are reported by Baümler, Rathery, Liebermeister, and Wilson.

Children convalescent from typhoid fever not infrequently contract one or other of the eruptive fevers. I have seen this occur repeatedly in the hospitals of Paris, where, at the time of my visit there in 1859–60, no attempt was made to isolate patients with infectious diseases.

BIBLIOGRAPHY.

General Treatises on Typhoid Fever.

Jaccoud, Traité de Pathologie Interne, tome ii. p. 768. Paris, 1874.

Aitken, Science and Practice of Medicine. London, 1866.

Harley, Reynolds's System of Medicine, vol. i. London, 1866.

Tanner, Practice of Medicine. Philadelphia, 1866.

Flint, Principles and Practice of Medicine. Philadelphia, 1868.

Wood, Practice of Medicine. Philadelphia, 1855.

Watson, Principles and Practice of Physics. London, 1871.

Liebermeister, Ziemssen's Cyclopædia of the Practice of Medicine, vol. i. New York, 1871.

Homolle, Nouveau Dictionnaire de Médecine et de Chirurgie, tome xxxvi. Paris, 1884. Article, Typhoid Fever.

Hutchinson, American System of Medicine, vol. i. Philadelphia, 1885.

Special Treatises on Typhoid Fever.

Cayley, On Some Points in the Pathology and Treatment of Typhoid Fever. London, 1880.

Collie, On Fevers. London, 1887.

Tweedie, Clinical Illustrations of Fever. Philadelphia, 1831.

Sutton, A History of the Disease usually called Typhoid Fever. Louisville, 1850.

Stokes, Lectures on Fevers. London, 1874.

Hudson, Lectures on the Study of Fevers. Dublin, 1867.

Nothnagel, Clinical Lecture on the Treatment of Typhoid Fever. Med. Press and Circular, March 9, 1887.

Flint, Medical News, April 3, 1884.

——, Clinical Reports on Continued Fevers. Philadelphia, 1855.

Peabody, Medical News, April 3, 1884.

Geissler, Bericht über den Typhus. Schmidt's Jahrbuch, Bd. cciv. S. 90, 1887.

Murchison, On the Continued Fevers of Great Britain, third edition. London, 1884.

Wilson, The Continued Fevers. New York, 1881.

Diseases of the Nervous System.

**Weiss*, Cerebral Abscess. Wiener med. Wochenschr., vol. xxxii. No. 50.

Shore, Muscular Atrophy and Gangrene of the Lung after Typhoid Fever. St. Bartholomew's Hospital Reports, vol. xxiii. p. 109.

Baümler, Klinische Beobachtungen über Abdominal Typhus in England. Deutsches Archiv für klinische Medizin, iii., 1867.

Ebstein, Sprach- und Coördinationstörung in Armen und Beinen in folge von Typhus Abdominalis. Deutsches Archiv für klinische Medizin, ix., 1872.

**Freund*, Inflammation of the Brain in the Convalescence of Typhoid Fever. Breslau, 1885.

Salesses, Amyotrophie Suite de Fièvre Typhoïde. Archives de Médecine et de Pharmacie Militaire, ix. p. 76, Jan. 1887.

Leyden, Klinik der Rückenmarks-krankheiten. Berlin, 1874.

Morel, Des Maladies Mentales. Nancy, 1852.

Simon, De lá Folie Consecutive à la Fièvre Typhoïde. Journal des Connaissances Méd. Chir., p. 53, 1844.

Thore, Observations d'Hallucinations Developées dans le Cours de la Fièvre Typhoïde. Annales Méd. Psych., p. 57, 1852.

Leudet, Remarques sur les Paralysies Essentielles Consecutive à la Fièvre Typhoïde. Gazette Médicale, p. 290, 1861; Thèse, 1851; Annales Méd. Psych., 1852.

Vulpian, Note sur deux Cas d'Accidens Survenus pendant la Convalescence de la Fièvre Typhoïde. Revue de Médecine, iii. p. 617, 1883.

Fernet, Nouveau Dictionnaire de Médecine et de Chirurgie, tome ix. Paris, 1869. Article, Convalescence

**Kraft-Ebing*, Beobachtungen und Erfahrungen über Typhus Abdominalis. 1871.

Landouzy, Des Paralysies dans les Maladies Aigues. 1880.

Nothnagel, Die Nervösen Nackrankheiten des Abdominal Typhus. Deutsches Archiv für klinische Medizin, ix., 1872.

Herbet, Considerations sur les Troubles de l'Intelligence qu on Observe dans la Fièvre Typhoïde. Archives Générales de Médecine, tome vi. p. 737, 5 series.

Pitres et Vaillard, Contributions à l'Etude des Nevrites Peripheriques Survenant dans

* References unverified.

le Cours ou la Convalescence de la Fièvre Typhoïde. Revue de Médecine, v. p. 985, 1885.

Putnam, J. J., A Case of Circumscribed Analgesia following Typhoid Fever. Trans. Amer. Neurological Association. Quoted by Revue de Méd. et de Chir., 2, 1878.

Gubler, Des Paralysies dans leur Rapports avec les Maladies Aigues, et Specialement des Paralysies Astheniques, Diffuses des Convalescents. Archives Générales de Médecine, xv. p. 402, 5 series.

Raymond, Deux Cas de Myelite Ascendante Observés pendant la Convalescence de la Dothienenterie. Revue de Médecine, v. p. 648, 1885.

Jones, Handfield, Abstract of Clinical Lecture on a Case of Paralytic Contracture after Fever. Med. Times and Gazette, p. 390, 1860.

Alexander, Ein Fall von Atropischer Lähmung der Beine nach Typhus Abdominalis. Deutsche med. Wochenschrift, xii. 529, 531. Berlin, 1886.

Deschamps, Abcès du Cerveau dans la Convalescence d'une Fièvre Typhoïde. Progrès Méd, xii. Paris, 1884.

Lonquet, De l'Aphasie Transitoire de la Fièvre Typhoïde. Union Méd., 3e. s. xxxvii. pp. 717–729. Paris, 1884.

**Duclour*, Aphasie, Hemiplegie et Hemianesthésie Transitoires dans la Fièvre Typhoïde, chez sur Garçon de Neuf Ans. Montpel. Méd., 2e. s. i. 478–485, 1883.

Tissier, Fièvre Typhoïde; Aphasie et Hemiplegie droite dans la Periode Terminale, Mort dans le Coma; Abcès Volumineux des Meninges. Progrès Méd., 2e. s., i. 440. Paris, 1885.

Courtade, Des Paraplegies Survenant dans le Cours ou pendant la Convalescence de la Fièvre Typhoïde. Encephale, vi. 431–470. Paris, 1886.

**Licharevski*, Deafness and Ataxia after Typhus. Russk. Med, v. 278 St. Petersburg, 1887.

Longstreth, Neuritis after Typhoid. Physician and Surgeon, ix. 201–205. Ann Arbor, Mich., 1887.

Pitres et Vaillard, Nevrites Parenchymateuses dans la Fièvre Typhoïde. Compt. Rend. Soc. de Biol, 8e s. ii. 661. Paris, 1885.

Waugh, Some Rare Psychic Sequelæ of Typhoid Fever. Trans. Med. Soc. of Penna., xviii. 189–195. Philadelphia, 1886.

**Zenner, P*, The Nervous Manifestations of Typhoid Fever. Amer. Lancet, N. S., xii. 9. Detroit, 1888.

**Pagliano, V*. Notes sur un Cas de Delire de la Convalescence dans la Fièvre Typhoïde. Marseille Méd, xxiv. 643–653, 1887.

Zenner, P., The Mental Disorders incident to Typhoid Fever. Cincinnati Lancet-Clinic, N. S., xix. 765–767, 1887.

Joske, A. S., A Case of Typhoid Fever Complicated with Tubercular Meningitis. Austral. Med. Journ, N. S., ix. 467–469. Melbourne, 1887.

**Piliotis*, De la Nevrite Periphérique du Cubital Consécutive à la Fièvre Typhoïde. Paris, 1885.

**Provost*, De l'Alienation Mentale Comme Complication Immediate ou Tardive de la Fièvre Typhoïde. Paris, 1885.

**Martel, E.*, Symptômes d'Ataxie Locomotrice dans le Cours et la Convalescence d'une Fièvre Typhoïde Grave chez une Enfant de 10 Ans. Rev. Internat. d. Sci. Méd., v. 2–4. Paris, 1888.

**Puybaret, Jacques A. C.*, Contribution à l'Etude des Paralysies dans la Fièvre Typhoïde. Bordeaux, 1887.

* References unverified.

Diseases of the Circulatory System.

Barié, Contribution à l'Historie de l'Arterite Aigue, Consecutive à la Fièvre Typhoïde, Revue de Médecine, iv. p. i., 1884.

Berthoud, Etude Pathogenique et Chirurgie sur l'Obliteràtion des Troncs Arteriels dans la Fièvre Typhoïde. Thèse de Paris, 1881; Revue de Médecine. iii. p. 155, 1883.

Mettler, Gangrene after Typhoid Fever. Philadelphia Medical Times, p. 339, Feb. 19, 1887.

Landouzy et Siredey, Contribution à l'Historie de l'Arterite Typhoïdique. Revue de Médecine, v. p. 843, 1885.

Fagge, Case of Embolism of the Pulmonary Artery in the Fourth Week of Enteric Fever. Trans Path. Society of London, vol. xxvii. p. 70.

Hayem, Leçons Cliniques sur les Manifestations Cardiaques de la Fièvre Typhoïde. Paris, 1875.

**Bordeaux*, Note Complementaire d'une Observation de Gangrène Spontanée des Extremités Inferieures dans le Cours d'une Fièvre Typhoïde. Arch. Méd. Belges, 3e. s., xxviii. 5–11. Bruxelles, 1885.

**Carpentier*, Fièvre Typhoïde, Endocardite Végétante des Valvules Sygmoides de l'Artère Pulmonaire, etc. Presse Méd. Belge, xxxviii. 233–235. Bruxelles, 1886.

**Parisot*, Recherches sur le Pouls dans le Cours, la Convalescence et la Rechute de la Fièvre Typhoïde. Nancy, 1884.

**Deschamps*, De l'Arterite Aigue dans le Cours de la Fièvre Typhoïde. Paris, 1886.

Landouzy et Siredey, Etude des Localisations Angio-Cardiaques Typhoïdiques, leur Conséquences Immédiates, Prochaines, Eloignées. Revue de Médecine, vii. 804–835. Paris, 1887.

Mettler, Arteritis complicating Typhoid Fever. Philadelphia Med. Times, xvii. 467–471, 1886–87.

Purkhiser, Typhoid Fever; Gangrene of the Foot. Progress, i. 362–364. Louisville, 1886–87.

**Facieu*, De l'Arterite des Membres dans le Fièvre Typhoïde. [Bordeaux] Toulon, 1886.

Landouzy, La Fièvre Typhoïde dans ses Rapports avec l'Appareil Vasculaire et Cardiaque. Gazette des Hôpitaux, lix. 323. Paris, 1886.

Guillon, Fièvre Typhoïde; Phlebite de la Veine Fémorale, Arterite du Membre Inférieur, Gangrene. Amputation, Guérison. Gaz. Méd. de Nantes, ii. 108, 1883–84.

**Alexander*, Typhus Abdominalis Complicirt mit Gangrän der Mundschleimhaut und Necrose der Oberkiefers; Tod. Breslau Aerztl. Zeitschr., ix. 271, 1887.

**Guinand*, Fièvre Typhoïde Terminée par une Gangrène de toute la Joue Droite. Loire Méd., vi. 298–300. St. Etienne, 1887.

**Ollivier, A.*, Chlorose Secondaire à la Fièvre Typhoïde. Semaine Méd., vii. 504. Paris, 1887.

Sudden Death in Typhoid Fever.

Deuèrre, De la Mort Subite dans la Fièvre Typhoïde. Archives Générales de Médecine, 2, p. 385, 1887.

Dieulafoy, De la Mort Subite dans la Fièvre Typhoïde. Paris, 1869.

Déjerine, Sur les Alterations du Myocarde comme cause de Mort Subite dans la Fièvre Typhoïde. Comptes Rendus Soc. de Biologie, p. 769, 1885.

* References unverified.

*Hardy, Mort Subite dans la Fièvre Typhoïde. Practicien, vi. 603–605. Paris, 1883.

*Wibin, Fièvre Typhoïde; Mort Subite, dans la Convalescence. Arch Méd. Belges, 3. s., xxvii. 308–311. Bruxelles, 1885.

*Barié, Pathogenie de la Mort Subite dans la Fièvre Typhoïde. Revue Gén. de Clin. et de Thérap., ii. 345–348. Paris, 1888.

Diseases of Genito-urinary System.

——, Orchitis in Typhoid Fever. Editorial, Lancet, vol. ii. p. 994, 1882.

Hamilton, Lancet, vol. ii. p. 1039, 1882.

Manly, Lancet, vol. ii. p. 1065, 1882.

Homolle, Nouveau Dictionnaire de Médecine et de Chirurgie, tomè xxxvi. Paris, 1884.

Byrd, St. Louis Med. and Surg. Journal, lii. p. 153, 1887.

Ollivier, Contribution à l'Historie de l'Orchite Typhoïde. Revue de Médecine, iii. p. 829, 1883.

*Larquier, De l'Orchite dans la Fièvre Typhoïde. Thèse de Doctorat, Paris, 1882.

Sadrain, De l'Orchite dans la Fièvre Typhoïde. Thèse de Doctorat, Paris, 1882.

Griesinger, Infectionskrankheiten, etc., Zweite Auflage. Erlangen, 1864.

Widal, Fièvres Typhoïdes Compliquées d'Orchite. Bulletin de la Société Clinique de Paris, tome i. p. 142, 1877.

*Sabourin, Orchite Parenchymateuse dans la Convalescence de la Fièvre Typhoïde. Bulletin de la Societé Clinique de Paris, tome ii. p. 229, 1878.

Hanot, Orchite dans la Fièvre Typhoïde. Archives Générales de Médecine, tome ii. p. 595, 1878.

*Chevedergne, De la Fièvre Typhoïde et de ses Manifestations. Thèse de Paris, 1864.

Duffey, On Rheumatic Orchitis as a sequel of Fever. Dublin Journal of Medical Science, vol. 53, p. 97.

Diseases of the Respiratory System.

Rendu, Fièvre Typhoïde Grave, etc, Suivie du Dévelopment Insidieux d'une Pleurisie Interlobulaire Suppurée, Ponction Aspiratrice, etc. France Méd., ii. 1809, 1821. Paris, 1885.

*Daville, Pleurisie et Perisplenite dans la Fièvre Typhoïde. Paris, 1886.

*Hublé, Tuberculose Consecutive à la Fièvre Typhoïde. Gaz. Méd. de Nantes, v. 31–33, 1886.

*Vazeille, Complication Pulmonaire de la Fièvre Typhoïde Simulant la Tuberculose. Paris, 1885.

*Finlay, D. W., Mild Attack of Enteric Fever, followed during Convalescence by Acute Tuberculosis. Middlesex Hosp. Rep., 1886, 43–45. London, 1887.

*Simon, De la Gangrène du Poumon dans la Fièvre Typhoïde. Revue Médical de l'Est, xix. 180–187. Nancy, 1887.

Lyman, General Tuberculosis as one of the Sequelæ of Typhoid Fever. Boston Med. and Surg. Journ, cix. 607, 1883.

Diseases of the Digestive System.

Dopfer, Ueber die Complicationen des Lethalen Abdominal Typhus. Münchener Medizinische Wochenschrift, Nos. 37 und 38, 1888.

* References unverified.

*Lockwood, Typhoid Fever; Death Eight Months after from Ulcers of the Intestines and Diseased Kidney. Med. Press and Circular, N. S., xii. 240. London, 1886.

*Stange, Perforation of Intestines in Typhoid Fever at Period of Recovery. Vrach, v. 732. St. Petersburg, 1884.

Michie, Abdominal Abscess following Enteric Fever. Brit. Med. Journ., i. 1388. London, 1888.

*Tordeus, E., Febbre Tifoide Sequita da Enterite Necrotica. Arch. di patol. inf., v. 257–260. Napoli, 1887.

*——, Fièvre Typhoïde Suivie d'Enterite Nécrotique. Clinique Bruxelles, i. 93–96, 1887.

*Gervais, Denis A., A propos de quelques Hepatites Consécutives à la Fièvre Typhoïde Observées en Nouvelle Caledonia. Paris, 1887.

Young, Extensive Contraction of the Ileum as a Sequel to Typhoid Fever. Medical Press and Circular, xlvi. p. 471, 1886.

Morin, Des Perforations Intestinales dans le Cours de la Fièvre Typhoïde. Paris, 1869.

Chauffard, Etude sur les Determinations Gastriques de la Fièvre Typhoïde Paris, 1882.

Meigs, A. V., Constipation. Transactions College of Physicians of Philadelphia, vol. viii., 1885.

Relapses.

Descroizilles, Recidive de Fièvre Typhoïde Six Mois apies une Première Atteinte, etc. Rev. Mens. d. Mal. de l'Enf., iii. 429–437. Paris, 1885.

Deshayes, De la Recidive dans la Fièvre Typhoïde. Gaz. Hebd. de Méd., 2e. s. xxiii. 637. Paris, 1886.

*Deumié, Elie, Contribution à l'Etude des Réitération de la Fièvre Typhoïde. Paris, 1887.

*Sallebert, Louis, Etude sur les Recidives de la Fièvre Typhoïde. Bordeaux, 1887.

*Rosenblatt, Ueber Typhus-Recidive. Göttingen, 1881.

*Devic, E., Des Rechutes dans la Fièvre Typhoïde d'aprés trente trois Observations Nouvelles avec Tracés Thermometriques. Lyon, 1886.

*Deroche, Note sur deux Observations de Fièvre Typhoïde à Rechute. Ann. Méd. Chir. franc. et etrang., iii. 113–117. Paris, 1887.

Anders, Case of Typhoid Fever in which Three Relapses Occurred, with Recovery. Med. and Surg. Reporter, xlvii. p. 66, July, 1882. Philadelphia.

*Perret, De la Rechute dans la Fièvre Typhoïde. Clin. Méd. de l'Hotel Dieu de Lyon, 475–496, 1887.

Hutchinson, American System of Medicine, vol. i. p. 303. Philadelphia, 1885.

Convalescence.

Hutinel, Etude sur la Convalescence et les Rechutes de la Fièvre Typhoïde. Thèse Presentée au Concours pour l'Agregation. Paris, 1883.

Fernet, Convalescence. Nouveau Dictionnaire de Médecine et Chirurgie, tome ix. Paris, 1884.

Rathery, Des Accidens de la Convalescence Paris, 1875.

*Jancts, Considerations sur la Convalescence des Maladies Aigues et sur les Accidents qu'elle pent presenter.

Szenic, Der Typhus Abdominalis und seine Folgezustände. Berlin, 1869.

Von Spitz, Ueber Polyurie in der Reconvalescenz von Typhus Abdominalis. Deutsche med. Wochenschrift, No. 8, 1882.

* References unverified.

Miscellaneous.

**Bernheim et Simon,* Fièvre Typhoïde d'intensité Moyenne; Abcès Pernephrétique Survenue pendant la Convalescence; Incision et Drainage; Guérison Complète apies sept Semaines. Rev. Méd. de l'est., xviii. 567–571. Nancy, 1886.

Boucher, Thyroidite Suppurée, Suite de Fièvre Typhoïde. Arch. de Méd. et de Pharm, Mil., vii. 353–356. Paris, 1886.

**Gilchenko,* Pyæmia after Typhoid Fever. Med. Obozr., xxvii. 1135–1143. Moskow, 1887.

Walker, Some of the Complications of Typhoid Fever. Med. and Surg. Reporter, liv. 98–103. Philadelphia, 1886.

**Abrial,* Contribution à l'Etude des Abcès Miliaries du Rein dans la Fiévre Typhoïde. Paris, 1887.

Delfin, Peritonitis en la Convalecencia de la Fiebre Tifoidea. Cron. Med. quir de la Habana, xii. 17–19, 1886.

Devine, A Case of Perinephritic Abscess following Typhoid Fever; Operation; Recovery. Boston Med. and Surg. Journ., cxvi. 422, 1887.

———, Fièvre Typhoïde de Longue Durée; Abcès Multiples; Phlegmatia Alba Dolens; Anémie Pernicieuse; Mort. Gazette des Hôpitaux, lix. 97. Paris, 1886.

Trélat, Abcès de la Fesse Consecutive à la Fièvre Typhoïde. Progrès Médical, 2. s., iv. 1067. Paris, 1886.

Frey. A., Ein Fall von Posttyphoser Contractur der Wirbelsäulemusculatur. Deutsche med. Wochenschr., xiii. 811. Berlin, 1887.

Leudet, Recherches Anatomiques et Cliniques sur les Hydropsies Consecutive à la Fièvre Typhoïde. Archives Générales de Médecine, vol. xii. 6 series, October, 1858.

Gairdner, Two Months Fever Duty in the Glasgow Royal Infirmary. Glasgow Medical Journal, vol. xii., 1865.

Church, Observations on Typhoid Fever. St. Bartholomew's Hospital Reports, vol. xvii. p. 97.

**Escherich, T., und Fischl, R.,* Ein Fall von Typhus Abdominalis mit Seltenen Complicationen. Münch. med. Wochenschr., xxxv. 21–23, 1888.

Ringer, Measles Occurring during the Course of Typhoid Fever. Lancet, i. 1295 London, 1888.

Archer, Cheesy Infarct of Spleen, Extensive Secondary Ulceration of Large Intestine, and Pleuritis in Enteric Fever. Brit. Med. Journ., ii. 448. London, 1886.

**Bernheim,* Fièvre Benigne, Lypémaine Anxieuse du 15e au 35e jour, Alimentation par la Sonde; Eruption Furonculeuse pendant la Convalescence. Rev. de Méd. de l'Est., xvii. 434–443. Nancy, 1885.

**Troisier,* Vegetures de la Peau dans la Convalescence de la Fièvre Typhoïde. Bull. et Mem. Soc. Méd. d Hôp. de Paris, 3e. s., v. 291–296, 1888.

* References unverified.

DISCUSSION.

DR. J. E. REEVES: I had hoped, from the title of the paper, the author would allude to some of the accidents which frequently occur. I have seen, without appreciable cause, during the third or fourth week, the patient suddenly seized with pain in the abdomen—in a word, with all the symptoms of perforation, and yet recover. I have witnessed seven such cases. Referring to the state of the mind and senses, I have on four or five occasions seen patients, after they were able to be out of doors for five or six days, seized with some hallucination, without permanent impairment of mind succeeding. I could furnish several examples of temporary derangement of mind following typhoid fever. The period of convalescence in typhoid fever is to me a most anxious one as regards the welfare of my patients.

DR. GEORGE L. PEABODY: I desire a word of explanation with regard to the administration of solid food to convalescents in typhoid fever. I believe that there are a number of patients to whom the administration of solid food would not be judicious, viz., patients who have suffered from severe attacks of the disease. Patients who are anæmic and markedly depressed in their nervous systems are not, as a rule, clamorous for solid food. These patients, I think, should not be given any solid food. On the other hand, there are patients who have had the ordinary series of symptoms of typhoid fever, lasting three or four weeks, who oftentimes regain their appetite and desire for solid food days before the fever has left them. As a rule, I think that with such patients it is safe to give regularly a small allowance of finely divided meat once or twice a day if they bear it well. I have done this several times where a recrudescence of the fever followed. This is not caused by the food, and is not in itself a reason for discontinuing solid food. I have in my experience met with a number of examples in which the administration of small amounts of solid food to patients immediately after the fever had left was not only not harmful, but has been of aid; and I do not think I have ever seen any bad results follow the practice. The administration of an egg is just as likely to do harm as finely divided meat.

Another point I would like to refer to, and that is the subject of ulcers as the cause of diarrhœa, and on this point I must differ from Dr. Hutchinson. I do not believe that the presence of ulcers bears any relation to the presence of diarrhœa. I have too often followed cases of typhoid fever to the autopsy table to agree with Dr. Hutchinson in this regard. Diarrhœa, I believe, is due to a catarrhal condition of the intestines, which may or may not be present in any given case. On this account it seems to me, if there be no diarrhœa present and no specially profound depression, it is perfectly proper to administer solid food soon after the period of fever has ceased.

DR. WILLIAM ORD, of London: I come from the other side of the Atlantic, and will put forward my experience with that of Dr. Hutchinson. In

regard to diet, I agree with him; but I am also in agreement, from frequent experience, with Dr. Peabody. I have been naturally under the influence of my colleague, Murchison, within the past few years. On more than one occasion of late, I have had warning that one must be guided to a certain extent by the desire of the patient for food, and I have learned to give into this desire in general. I will quote a case which occurred a month or two ago. It was a girl in St. Thomas's Hospital, who had a severe attack of typhoid fever, and convalescence had set in. Then, without any existing condition to account for it, she began to have a large range of temperature, rising steadily in the evening and falling in the morning, till at the end of three days the range of temperature was from three to five degrees. With this she became slightly delirious, and menaced her physician when he came around. Then, for two or three days more, the temperature was still higher, with more disorder in her mind and craving for solid food; at the same time she refused to take liquid food. Now I tried the effect of giving her solid food. She was allowed at once boiled sole. Within twenty-four hours after taking the fish her temperature became normal, her delirium ceased, and from that time on she made an uninterrupted recovery. This is an instance of what we have to do in all cases of convalescence, viz., the study of the individual as well as of the fever.

With regard to diarrhœa, I would say there is a good deal of reason in Sir William Jenner's explanation that where diarrhœa is conspicuously present there is also a conspicuous presence of ulceration in the large intestine, where it is apt to be associated with extensive catarrh. Certainly, in epidemics which I have watched from year to year in London, the existence of these things has been very notable. Where there is much diarrhœa, there is considerable ulceration existing in the large intestine, which post-mortems have enabled us to verify. This bears also on the question of relapse. It was only ten or twelve years ago that many relapses were noticed in London. Up to that time they had the typical form of diarrhœa, then they had epidemics in which constipations were more marked, and then the trouble of relapses, by which I mean not recrudescence, but a complete second attack. When constipation existed, they used to give castor oil. After long study of these cases, I have come to the conclusion that the relapses were, in all probability, due to an infection from within; that long constipation had allowed time for germs to produce a second infection.

There are one or two points in relation to sequelæ I would like to ask Dr. Hutchinson. Very often, in long-continued cases of typhoid fever, we have periostitis and phlebitis, and I think deep ulceration in the larynx is a common occurrence. I would like to know if these ulcers have been often observed in this country. Phlebitis is a thing that always puzzles me, and with regard to which I would like to have some information.

DR. F. P. KINNICUTT: I believe that relapses in typhoid fever are simply the result of re-infection and not of imprudence in diet. I believe we

frequently see recrudescences of many of the symptoms from imprudence in diet, and it seems to me that, although there are, undoubtedly, cases carefully selected in which solid food may be safely given and possibly with benefit, shortly after the disappearance of the fever, there are, also, cases in which solid food may work ruin to the patient. The advice, if given generally, would be abused, and I think the rule mentioned by Dr. Hutchinson is the best one for the profession to follow—careful regulation of diet during the first ten days.

DR. F. C. SHATTUCK: Mention has been made of relapses, and also of recrudescence. I have noticed, during the stage of convalescence in typhoid fevers, a persistent rise of temperature, which was neither recrudescence nor relapse, and which did not disappear until the patient got out. It seemed to be a bed fever. I am satisfied of the existence of this fever, and think it important that we should recognize its existence, and not be alarmed by it. The more we see of temperature the more it is forced home to us that it is not to be taken as a single guide for action, but is to be regarded as only one of the factors for the solution of the problem before us.

DR. J. C. WILSON: The question of diet, during the early period of convalescence, is one of such importance, and such divergent views have been expressed here, that I venture to ask Dr. Ord, in regard to the single case he related, as to the diet of the patient prior to the mania he spoke of. Is it not possible that this patient's mania might have been due to defective nutrition; in fact, a mild form of starvation mania?

DR. WILLIAM ORD: Previous to the administration of solid food the patient had been given beef tea and milk, in the proportion of two parts of beef tea to one of milk, as freely as the patient could take it, and six days before the occurrence of the maniacal symptoms two eggs a day had been given in addition; but she had refused all these for a day or two.

DR. HUTCHINSON: The sequelæ of typhoid fever are very frequent in this country, as shown by Dr. Keen, of Philadelphia, in his Toner Lecture. I have alluded to several of them in that part of my paper which I have not been able to read on account of the limit of time imposed by the Association. Among them are several diseases of the nervous and circulatory systems. Attention has also been called somewhat prominently of late to the fact that orchitis is an occasional complication of this stage. This has been ascribed to masturbation by Bucquoy, who asserts that it is frequently practised by patients during convalescence from this disease, when the genital functions are said to be specially active; but a much more probable explanation is that which refers it to thrombosis of the spermatic vein. Dr. Da Costa some years ago called attention to the fact that a low grade of fever occurred in convalescence, which was cured by the patient's being ordered out of bed. On the other hand, a recrudescence will occur if the patient gets out of bed too soon.

ON SOME FORMS OF PARALYSIS AFTER TYPHOID FEVER.

By GEORGE ROSS, M.D.,
PROFESSOR OF CLINICAL MEDICINE, MCGILL UNIVERSITY, MONTREAL.

In a short communication relating chiefly to some of the surgical sequels of typhoid fever, Sir James Paget says:

"I do not feel competent to deal with the question whether each fever has, as seems probable, its own proper sequels, and in this sense, though perhaps in less degree, as specific as in its fever-period; but I have not yet seen either periostitis or local paralysis of muscles after any other than typhoid fever, or a fever which I suppose to be closely related to it, and I have not seen after typhoid any corresponding number of cases of large lymph glands, diseased joint, or other diseases of mere debility, such as may follow any acute illness."

And he quotes the remark of Dr. Baly, to the effect that "a book on the diseases of convalescence might be one of the most valuable that any physician could write." A consideration of the truth of this remark has led me to lay before the Association a few notes upon the important subject of paralysis following typhoid fever.

It is an interesting question how far these so-called sequels or well-defined disorders occurring during the convalescence from fevers and other acute diseases are special or confined to certain of them only, and how far others may occur in a similar relation to several of them. Certainly, in many instances, the latter is found to be the case, as, *e. g.*, otitis media after both scarlatina and measles, whilst, again, some peculiar local derangements seem to follow in the wake of a particular fever, and to bear no similar relations to any other, *e. g.*, paralysis of the palate following diphtheria. So with periostitis after typhoid, which may occur in many situations. I have seen it several times on the tibia, once leading to severe necrosis, often on the ribs, and twice on the surface of the sternum. So with the affections of the nervous system, the most common form being of the muscles supplied by the

peroneal or anterior tibial nerve, whilst more extensive paralyses are decidedly uncommon. The very peculiarities of the parts affected, and the frequency with which they are attacked in preference to all others, show certainly a predilection for them which must give the stamp of specificity.

It is not to be wondered at that the nervous system suffers in many ways after the prolonged disturbance to which it is subjected during a siege of typhoid fever. Indeed, it would be remarkable if derangements of the nervous functions were not often witnessed under these circumstances. In a certain proportion of all cases of this fever, the incidence of the poison seems to be upon the nervous system, as is evidenced by the occurrence of cephalalgia, insomnia, delirium, tremors, and debility. As a sequel of these profound and prolonged disturbances in the nervous system all patients who have thus suffered are left in a condition of very marked asthenia, and it is a long time before they are in a condition to bear any, even moderate, amount of either bodily or mental fatigue. These signs of exhaustion of the nervous system are constant, and generally in proportion to the severity of the fever, and the degree to which its violence has fallen upon that portion of the mechanism. But, apart from such general functional diminution, there are, in exceptional cases, signs that certain isolated, and often strictly localized, portions of the nervous system have suffered out of all proportion to the rest, and in them the impairment of function may reach a high degree. Thus, the cerebral cortex may be the part chiefly affected, and we observe, even after full convalescence has set in, that the mental functions remain imperfect and hallucinations and delusions may persist for a very considerable time—or, as concerns us most at present, some part of the spinal cord, or some one or more of the spinal nerves, exhibits altered function.

It is, however, a fact that, although these sequential paralyses are more frequently observed after somewhat severe attacks of typhoid, yet they do not specially occur when the fever has been marked by an excess of nervous disturbance. Nor can any such predisposing cause as the influence of country or of a neurotic temperament be shown to play any part. The nervous phenomena, almost invariably, are both motor and sensory, presenting paralysis with either pain or anæsthesia. This has always been observed in *spinal* nerves, and has not been seen in any of the mixed (motor and sensory) central nerves. According

to Nothnagel (*Deutsch. Archiv für klin. Med.*, 1872), from an examination of recorded cases, the following is the order of frequency of these affections: 1. The parts supplied by one nerve or branch of a nerve, with special predilection for the ulnar and the peroneal. 2. Paraplegia, generally confined to the lower extremities, but not very infrequently involving an upper extremity—sometimes both arms and legs—sometimes one side more than the other. 3. Less frequently, one extremity, either upper or lower, or two extremities in crossed order. 4. Simple alterations of sensibility; if looked for, these will be found *very frequently*, though not so striking as the cases with combined paralysis and anæsthesia. They are observed chiefly in the lower extremities and especially in the feet.

From a consideration of many cases of paralysis after this fever, it may be inferred that the mode in which it is produced is by no means always the same; that, indeed, the effect of the poison is exerted sometimes upon some of the great nervous centres, especially of the spinal cord, and at other times upon the structures of some special and individual nerve or nerves. Jaccoud, *e. g.*, speaks of paraplegia being due to *œdematous infiltration* of the parts in the vertebral canal, and is of opinion that even prolonged *exhaustion* of these centres without organic lesion may be the cause of a motor loss enduring even for a considerable time. Prof. Biermer relates an important case, in which he finds difficulty in deciding between acute poliomyelitis anterior and multiple degenerative neuritis, there having been pain, motor paralysis, good sensation, diminished electrical reaction, atrophy, and bedsores, a tedious illness of several months, and, finally, complete recovery. As the tendency in all these cases is toward recovery, their pathology is necessarily scanty and founded upon what is better known about recognized lesions whose symptomatology is somewhat analogous.

The commonest variety of post-typhoid paralysis is that which is confined to the district supplied by one nerve, *e. g.*, the ulnar, peroneal, or the circumflex. Here, it is extremely probable that two distinct pathological conditions may exist. There may be a true interstitial neuritis or there may be an acute parenchymatous change in the nerve elements. The two varieties are separated from each other mainly by the presence or absence of *pain*—this symptom being a marked feature in the former, owing to the pressure exerted upon nerve filaments, and being absent in the latter where this condition does not

exist; the main feature, however, the loss of power in the supplied muscles, being equally present in them both. It has been suggested by a recent writer (Thomson) that a similar acute parenchymatous degeneration is to be looked upon as explaining that common post-febrile paralysis, the diphtheritic, and the view has much to commend it.

It is not unusual after typhoid fever of considerable severity to find a definitely enfeebled condition of the lower extremities persisting for some time, and sometimes a person never entirely recovers his capacity for walking long distances. Such paretic cases have never been specially studied, but it is probable they would, if any should fall under the head of defective innervation from prolonged exhaustion of the nervous centres.

The most important, however, of cases of post-typhoid paralysis are those of paraplegia, confined sometimes to the lower extremities and sometimes involving these together with some other parts. Well-marked cases of this kind are sufficiently unusual, and one may see several hundreds of cases of typhoid fever without meeting with a single example. Not much has been written upon it, and in some articles very dissimilar conditions are probably included under the same description.

The following case is a striking instance of a very severe form of paraplegia directly resulting from typhoid fever and still ending in complete recovery.

Julia L. was brought by the ambulance to the Montreal General Hospital on the 8th of November, 1887. The following account of her illness was obtained from her attending physician. She complained on September 1st of headache, feverishness, pains in the limbs, and a short hacking cough. After one week took to her bed. Was first seen by her physician on September 12th. At this time, she had a severe cough with whitish expectoration; moderate distention of abdomen, and gurgling in right iliac fossa. Temperature 102° F. The disorder ran the ordinary course of a moderately severe attack of typhoid fever without complication. Patient remained in bed until October 25th, when she got up, went about the house for three or four days and was apparently convalescent; appetite good, sleeping well, and gaining strength rapidly. On October 29th temperature rose to 102° F. and she began to suffer from severe pains in the legs. The lower extremities were very sensitive, the patient not being able to bear any motion of the bed-clothes. The legs were flexed on the thighs and the thighs on the pelvis, any attempt at extension being very painful. At this time had incontinence of

urine for one day. The skin over the left trochanter and sacrum very soon became red, and in two or three days a large slough, about two inches square, had formed in both these situations. The skin over the right trochanter also became reddened but did not slough. The temperature soon became normal and remained so.

On admission, much emaciated and anæmic; face pale; eyes brilliant, pupils dilated and equal; expression anxious and haggard; skin dry and harsh, cool; tongue clean and moist; abdomen retracted, not tender on pressure. Lies on right side with legs and thighs flexed. Some œdema of left foot. Numerous small purpuric spots on both legs from knees downward. Great wasting of muscles of both lower limbs, thigh and leg; both knees are rigidly bent; any movement to relieve the contraction is extremely painful. Marked paresis in all attempted movements of foot, leg, or thigh. She is unable to hold up either leg from the bed without support. Much tenderness on pressure upon the muscles of the legs and thighs. No anæsthesia. The skin over the two malleoli somewhat reddened. Plantar and abdominal reflexes present. There is a large bedsore over the lower part of the sacrum, about two inches square, covered with a hard, black slough, and with inflamed edges. There is a similar slough on the left trochanter, and the skin over the right trochanter is reddened but unbroken. Pulse feeble and rapid, but regular. Temperature normal. In lungs, many moist sounds heard at both bases, chiefly in the left. Urine contained a small amount of albumen. Complains of pain over the bedsores and weakness. Eats well. Sleeps soundly. Urine and feces passed naturally. Is very feeble and resents being moved or stirred.

She was put upon a water-bed and the bedsores poulticed, and was given quinine, good diet, and a small allowance of stimulants. The sloughs soon separated and the sores were dressed with balsam of Peru and iodoform; they slowly granulated and finally healed. The contraction of the knees obstinately remained. In February, the legs were gradually extended by means of weights and pulleys. By this means they were brought down until the legs were quite straight. During this time, motor power was slowly returning and pain quite disappeared. After a time, she could stand upright, and perform flexion and extension movements of the feet and legs. Improvement was then rapid, and she was discharged March 1, 1888, able to walk nicely about the ward. She has since remained well and strong.

During apparent convalescence there occurred severe pain in the limbs, marked tenderness of skin and muscles, no anæsthesia, diminution of superficial reflexes, loss of electrical contractility, extensive bedsores, loss of power to a great extent but never complete, contractures, no disturbance of sphincters. The case looked very unpromising and it was only the known tendency of these cases toward recovery and the probability that the lesion was really in the nerve-trunks,

and not in the spinal cord itself, that enabled a somewhat favorable opinion to be entertained.

The second illustrative case is still more remarkable inasmuch as it presents an example of unusually extensive paralysis after typhoid—the nervous disorder involving all the limbs, and, in addition, the palatal muscles after the manner of diphtheritic palsy.

Ernest H., æt. fifteen years, was admitted under my colleague, Dr. Wilkins, into the Montreal General Hospital on the 26th of March, 1888, suffering from typhoid fever of a few days' standing. The case presented no unusual features, and was of a rather mild type. On the twenty-fourth day of illness the temperature was 99°, and in a few days later it reached and continued at the normal figure. About this time he found that he spoke with some difficulty—that is to say, the effort required for speech was greater than it should be; it was noticed that his speech had a nasal intonation. There soon followed a sense of numbness in the ends of the fingers and in all the limbs; the weakness being greater upon the left side than the right. By the 4th of May he lay in bed without power to raise either leg from the bed; the head and arms were raised with great difficulty, and the grasp was very feeble. There were no pains in the extremities. The voice was quite nasal, speech was difficult, and swallowing required an effort and occasionally produced regurgitation. The velum palati was relaxed and insensitive. Knee-jerk absent in both legs. Tactile sense good. Muscles of lower extremities react normally to the faradic current, but somewhat slowly to the galvanic current. Muscles of the upper extremities react three times as strongly on the left side as on the right side. Eyes examined by Dr. J. J. Gardner: "pupils react to accommodation and light, but somewhat lazily to light. Accommodation unimpaired. External ocular muscles act well, no weakness of internal recti. Fundus normal." He remained in this condition of extensive paresis, under treatment by means of galvanism and strychnia for several weeks, being removed by his friends on the 27th of June, 1888. At this time the limbs had improved in power to a moderate extent, and the nutrition was better; the voice was less nasal.

Gowers (*Diseases of the Nervous System*, vol. ii.) alludes to similar cases, but mentions that, as diphtheria is known sometimes to accompany typhoid fever, so a paralysis in reality of diphtheritic nature may be observed as a sequence of such a compound attack; and the question may be raised whether H. did not have diphtheria. There is no proof that he did have throat affection of any kind; and, on the other side, he was, during the whole time, under skilled attendants in a hospital, and nothing was ever noticed or complained of which led to an examination of the fauces. It may, therefore, I think, be safely

held that no diphtheria complicated the case. I would also recall, as strongly corroborative of the same view, that there was no disturbance of accommodation, and no albumen in the urine.

Although actual observations are not very numerous showing presence of the lesions of peripheral neuritis, and the absence of change in the spinal centres in cases in which post-typhoid paralysis has existed, yet they are sufficient to substantiate the occurrence of such a disorder. Indeed, in the light of the somewhat remarkable observations recorded by Pitres and Vaillard (*Revue de Médecine*, t. v., 188.), we may well be surprised that the clinical evidences of neuritis are not more frequently met with. These writers made careful and extensive histological examinations of peripheral nerves in various parts of the body in cases of typhoid fever which had proved fatal from various causes, but in none of whom there had, at any time, been evidences of nervous lesion or disorder. The result was, in every case, to find microscopical evidence of well-marked changes in the structure of most of the nervous trunks and their branches, whilst the actual nerve-roots and the spinal centres remained unaltered. And the question is raised, "How frequently in reality does such neuritis occur?" In the examples recorded there had been nothing from a clinical standpoint to suggest its existence, and they were selected quite by chance. Does it occur both in fatal cases and in those mild or more severe cases which end in recovery? We are led to think the latter possibly true, owing to the frequency with which more or less marked evidences of sensory, motor, or trophic disturbances are met with as sequences of typhoid fever.

Is this neuritis set up by the elevation of temperature? or by the general derangement of nutrition? Or may the typhoid poison act directly upon the nervous fibres, irritate these, and ruin their structural integrity—determine, in fact, a parenchymatous neuritis? This neuritis, it is stated, attacks not only the superficial branches which innervate the skin, but also the larger and deeper trunks. May it not, therefore, be that the disturbances in them are to blame for many of the symptoms generally observed in typhoid: fever cutaneous hyperæsthesia, muscular hyperæsthesia, wandering or localized pains in the limb, etc., symptoms which are often attributed, perhaps rather gratuitously, to irritation of the meninges or of the spinal cord? The truth would seem to be that when a neuritis of this kind is mild in

character and affects only a few of the fibres of any particular nerve, no appreciable symptoms are produced thereby, or, at any rate, such slight phenomena as it may induce are lost in the complex symptomatology of the pyrexia; and that, for some obscure reason, these nervous lesions sometimes become exaggerated, extend to still other bunches of fibres, and then give rise to the well-marked symptoms due to a diffuse or a localized neuritis: sensory, motor, and trophic disorders, confined to the region supplied by one or several nerves, according to the distribution of the determining lesions.

As regards the *etiology* of this affection, some authors have regarded it as due to the anæmia which must necessarily result from a long febrile illness. That such is not the case is proved by the actual condition of many of the patients who have shown no such degree of blood-impoverishment as would be at all sufficient to account for serious disorder of the nervous centres or their branches. The series of cases produced by Nothnagel were selected from the barracks, and were all young, vigorous, and full-blooded men. The effects of any such anæmia would surely be more general, involving loss of energy in movements, but not a true paralysis. It could not possibly produce the clearly defined paralysis affecting special nerves as we find after typhoid fever. The other hypothesis which has been suggested is that the disorder consists essentially in a degeneration of the affected muscles. That this cannot be, is sufficiently obvious from the extensive sections of the body which may be simultaneously involved, as, *e. g.*, both lower extremities, or these together with an upper extremity; from the constant presence of pain at some period, and often severe; and from the existence of anæsthesia.

Post-typhoid paraplegia generally sets in gradually and disappears gradually, but, in rarer examples, it comes on suddenly and passes off either suddenly, or, at any rate, very quickly. Such a case is that reported by Ollivier, which led him to infer the existence of a true, but temporary, congestion of the spinal cord. There is nothing impossible about the hypothesis, and it seems necessary in order to explain such a case, but certainly does not apply to the more common forms of gradual development. Nothnagel suggests that actual spinal hemorrhage is more probably here present, and that meningeal hemorrhage has, in reality, been often found in fatal cases of typhoid fever.

The predilection for certain special groups of muscles observed in

the paralyses of plumbism and of diphtheria does not exist in the same degree after typhoid fever. In it there is no rule, and, to explain it, we must look for some cause capable of making its influence felt in an infinite variety of situations: the ulnar or the peroneal nerve, a plexus of nerves, both lower extremities, an upper, together with the opposite lower, extremity. At the same time, all degrees of intensity may be observed, from the slightest paresis up to complete abolition of function.

The paralysis generally develops slowly and gradually, and it commonly advances to a considerable degree before being arrested. Arrived at this stage, it may be expected to recede spontaneously or under the influence of treatment, and the case to end in complete restoration of function. A more unusual course is that in which, as before, a slight, or very moderate degree of weakness is gradually developed, and then suddenly a great change occurs, and a perhaps complete paralysis sets in. Even here, the usual result is gradual return of power. In either case, there are exceptional instances in which the paralysis will be permanent, even in spite of early and efficient treatment.

The first symptom is generally pain, and this may precede any motor defect by some considerable time. Pretty often, the pain and the commencement of the paralysis are noticed about the same time.

The cutaneous sensibility is nearly always diminished, and these sensory disturbances—*i. e.*, pain and anæsthesia—are amongst the most constant phenomena of this affection. In severe and well-marked cases trophic changes are seen, consisting chiefly in a greater or less degree of atrophy. There is diminution of electrical contractility, both to the galvanic and to the interrupted current.

From a consideration of these general features Nothnagel strongly argues that the condition is essentially the same as one of traumatic paralysis when the injury has been done by some gradually compressing cause. We must, in that case, look for some pathological cause which is capable of acting in this way. Certain it is that, in fatal cases of diphtheritic paralysis (which has many affinities with that under consideration), there has been found evidence of thickening of the anterior and posterior roots of the involved nerves, especially in the neighborhood of their point of junction; and also of diphtheritic infiltration within the nerve-sheaths. It is probable that there would

be found, in an early stage, proliferation of the connective tissue, and that the subsequent contraction which always sets in under these circumstances would keep up the compression of the nerve-filaments. The same hypothesis would apply to examples of extensive— or even quite limited—areas of anæsthesia.

DISCUSSION.

DR. WILLIAM ORD, of London: There are some things in the paper which interest me very much. I cannot say that I have seen many of these cases of peripheral neuritis. They are rare in my practice. I have observed indications of cerebral weakness, long-continued imbecility, remarkably protracted silence, and difficulty of articulation. I have seen cases after a very severe attack, in which there has been a long period of comparative unconsciousness, in which the patient has spoken at the rate of a word every two or three seconds; and in more than one case I have seen a patient come out of a condition of unconsciousness, be unable to speak at all, and recover the power of speech only after several days. In one case, of a marked degree, I found the patient unable to speak at first; he had, in fact, lost the faculty of remembering words and had to learn to speak over again. This is more apt to occur in young children from seven to eight years of age, and in one case I have seen the process of learning to speak have to be recommenced and developed as originally. I think all these changes are rather due to changes in the central nervous system than in the periphery. I have found it convenient in dealing with these and other sequelæ, as well as the complications of enteric fever, to classify them in my own mind. I have been accustomed to classify them, first, under the head of complications, which are, so to speak, inherent in this particular kind of fever, and probably those forms of neuritis and cerebral weaknesses are among those which are characteristic. Then I should take those belonging to fever in general, and then those complications and sequelæ which are accidental and to a large degree personal. It has been noted by one of the speakers that typhoid fever probably awakens latent mischief in those cases which are followed by phthisis. I think there is good reason to believe that it has the general power of arousing existing mischief, which was up to the time veiled. In people of rheumatic tendency, we will find it awakening a rheumatic condition. It seems to have the quality of bringing out things which were masked and hidden before.

DR. FRANCIS MINOT: One idea which struck me on hearing Dr. Ross's paper read, was the possibility of these different sequelæ of typhoid fever being caused by more than one germ entering the system. On the other

hand, there are certain diseases which apparently have the same microbe; as, for instance, pneumonia and cerebro-spinal meningitis. The germ from the healthy human saliva will produce pneumonia in an animal, and in its tissues will be found the true pneumonic germ; and possibly some of those additions to disease may be caused by the same germ entering afterward. Among the sequelæ of typhoid fever is dumbness, which may be due to some other germ acting after the fever has run its course. Some time ago I saw a patient of eighteen who for some weeks was unable to speak a single word after a severe attack of typhoid fever.

DR. M. ALLEN STARR: It is pretty well admitted that neuritis may be due to infectious causes, and yet it seems to me that a word of caution may be uttered in regard to ascribing all cases that follow typhoid to the influence of the typhoid germ. It seems to me that if all the cases of neuritis following typhoid were due to the typhoid infection, neuritis would be much more frequent than it is. I have looked into the matter somewhat, and find that neuritis is a very infrequent sequel of typhoid fever. I have seen within the past few years cases of general peripheral neuritis following typhoid. In all cases they were possibly alcoholic. We know that alcohol is one of the frequent exciting causes of multiple neuritis, and it is doubtful whether neuritis following typhoid is due to the typhoid germ, or to alcohol taken prior to or during the disease.

There is another class of cases to which Dr. Ross alluded, viz., those in which the neuritis is limited to one or two nerves. Is it not possible in those cases that, instead of the neuritis being due to any infection, it is simply the result of pressure upon those nerves? I have had three cases lately under observation of distinct peripheral neuritis limited to one nerve, which were due to pressure of the body upon the bed while the patients lay in a comatose state. Could not the limited neuritis of typhoid fever be explained by this means? It seems to me that this question might be answered by some of the gentlemen present. I do not think that Dr. Ross's cases belong to that form of typhoid fever in which there are central changes in the cord, but myelitis is admitted as a sequel of typhoid fever. These cases must be, I think, very carefully distinguished from those with marked signs of general multiple neuritis.

DR. WILLIAM OSLER: I think that those who have followed the pathological history of late paralysis will bear me out in stating that writers are divided into two classes, one stating that the late palsy is due to a neuritis, and the other that it is due to a poliomyelitis. This is also true of arsenical poisoning. Both clinical and experimental evidence seem to me to agree upon the fact that we may have a toxic myelitis or a toxic neuritis. Now in this disease it is the same; we may have typhoid fever lighting up a myelitis or a neuritis.

DR. H. C. WOOD: Those who have studied the course of lead palsy have had to deal with this same problem, finding either central or peripheral

paralysis, myelitis, or neuritis; it is evident that the poison in some cases attacks the nerve centres, and in others the peripheral nerves, but we are not able to say why sometimes one, sometimes the other of these localities is selected.

DR. FRANCIS DELAFIELD: Although the lesions that have been described are not, after all, very common, yet they are such as belong to the disease. It seems to me that the poisons of the severe infectious diseases behave in very much the same way as some of the mineral poisons; that is, they produce an acute degeneration of certain parts of the body, and with this acute degeneration there may or may not be added inflammatory changes. And the poisons of the infectious diseases have the same capacity for producing acute inflammation that the mineral poisons have, but they differ as to the parts affected; thus, in diphtheria, it is the nerves that are affected; in typhoid fever we find the spleen swollen and changed; and so on, as we review the different diseases. That would seem to me to be the condition of things in typhoid fever, as regards the nervous system.

DR. GEORGE ROSS: I have confined myself entirely, in my paper, to an account of those forms of paralysis which are of common occurrence, and have avoided any reference to the cerebral manifestations occasionally met met with in typhoid fever. I have seen cases which bear out the remarks made by Dr. Ord, and especially have I been interested in those severe forms of mental derangement which come on during convalescence, and last for an unexpected length of time. The possibility of alcoholic neuritis is universally admitted. I could not say that the matter had been looked into in the cases reported by me. That an alcoholic neuritis could be set up by partaking of small quantities of alcohol in an illness extending through eight or ten weeks was, I think, unreasonable to suppose. As to the theory of pressure as a possible cause of neuritis, of course it was only in certain cases, such as those mentioned, that the theory could be entertained. Against it there is, in the first place, the fact, alluded to by several speakers, of the existence of several forms of neuritis after various febrile diseases, and the greater degree of severity of these forms of paralysis than in that after pressure upon a nerve for a certain time. The facts that certain of these cases end in atrophy, and that some of them are actually permanent are, I think, against the idea that a neuritis of such severity has been set up by pressure on a nerve. I think we should endeavor to make a distinction between myelitis and neuritis. I saw a few years ago, in an epidemic of smallpox, some cases of myelitis, and the prognosis in such is very bad. The disease progresses from bad to worse and a fatal result may usually be looked for.

FATTY OVERGROWTH OF THE HEART.

By F. FORCHHEIMER, M.D.,

PROFESSOR OF PHYSIOLOGY AND CLINICAL DISEASES OF CHILDREN IN THE MEDICAL COLLEGE OF OHIO, CINCINNATI.

It appears almost needless to state that fatty heart means two different forms of pathological changes—the one an infiltration, the other a degeneration. The object of this paper is to discuss the former, fatty overgrowth of the heart.

In looking through the literature upon this subject, it is remarkable how many names have been given to this affection; thus, Gowers (Reynolds's *System of Medicine*, vol. iv. p. 760) calls the affection fatty overgrowth, and then gives the synonyms as follows: fatty infiltration (Rokitansky); fatty overgrowth, fatty hypertrophy (Quain); adipose cardiaque, surcharge graisseuse, obesité du cœur (French writers), to which can be added lipomatosis, polysarcia cordis, cor adiposum of the Germans, not to mention the German names themselves, which are used by the latter. The term "fatty heart" is certainly not sufficiently distinctive, as others (Pepper) have already pointed out, and, as we are dealing with an affection clinical principally in its importance, it would be well to unite upon one term, so that the confusion which now exists might be done away with. The name chosen is one of two given by Richard Quain (*Medico-Chirurg. Trans.*, 1850, p. 121 et seq.), the author who has written if not the best, certainly the most memorable article upon the subject; it seems to express as much as need be expressed. If we chose to retain short terms, fatty heart might be retained for the degenerative process, and fat heart for the infiltrative.

So far as the historical development is concerned, it certainly is a fact that Hippocrates (the all-knowing), Galen among the very old, and Morgagni, Senac, Haller, Lancisi, and others among those preceding the present modern school, have referred to the subject in one way or another. Laennec and Corvisart spoke of both fatty degeneration

and infiltration, but made nothing of the condition from a clinical standpoint. For the first appreciation of this condition, not only at the post-mortem table but also at the bedside, we must turn to the Dublin school, represented by Cheyne (1818), Stokes a little further on, and Kennedy, who is very diffuse (1849–50). By far the best and most acute essay written about this time (1836) is that of Latham (*Lectures on Subjects connected with Clinical Medicine*, published by the New Sydenham Society, 1876, vol. i. pp. 326–328), which we will have occasion to study a little more carefully. He makes a distinct division of fatty heart into obesity and degeneration, describes correctly the changes undergone by the muscular tissue, gives an excellent though very brief account of the symptoms, and places an estimate upon the possibility of a diagnosis which, certainly, seems very sound. Next, chronologically, comes the essay of Richard Quain, accompanied by a table of cases which forms the basis of a table published by Hayden (*Diseases of the Heart and Aorta*, Philadelphia, 1875), and is, in every respect, the most important contribution to the subject in the English language.

During all this time the Germans had been quiet, from a clinical standpoint, and it was not until 1878 that we find Leyden beginning his work (*Berl. klin. Wochenschrift*), which he continued and completed in 1882 (*Ztschrft. f. klin. Med.*). Oertel (*Hdbch. d. allgemeinen Therapie*, vol. iv., 1884), who has made special studies and reduced the treatment to a method, claims that he began his work nine years before publication and, although priority as far as German literature goes must be denied him, he, with Leyden, has done more to call the attention of the profession to fatty overgrowth of the heart than any one else.

The large handbooks which have been published in our days (Virchow, Ziemssen, Pepper) all devote more or less space to the subject and it is a special gratification to see that Prof. Osler, in the discussion of the subject in Pepper's *Cyclopædia*, has held to the strict line of demarcation between fat and fatty heart. Most modern textbooks refer to the subject, but, as a rule, they discuss both conditions together. There have been but two complete tables published (Quain and Hayden). It was found very difficult to decide upon proper cases, and, for the sake of accuracy, only such cases have been included in which the diagnosis was beyond doubt. A great many of the older

cases, therefore, were rejected on account of incomplete reports, and other cases, from which incomplete deductions only can be drawn, were also omitted.

ETIOLOGY.—Whatever causes obesity may cause fatty overgrowth of the heart. The two conditions are absolutely connected together. When authors speak of the occurrence of a fat heart in phthisis or cancer, it may be a process of fatty degeneration, or of true fatty overgrowth; in the latter case due to obesity, however. Quain states that it is more common in males than females, but quotes Bizot, who found it more common in females. The latter, according to Quain, found 9 fat hearts in 14 fat females, and 14 fat hearts in 29 thin females; but, in the language of Quain, Bizot's investigations apply to "a mere greater or less quantity of fat." In the table appended, 88 cases were males and 34 females. As far as age is concerned, it may occur at almost any time of life. Thus Kisch gives 36¼ per cent. from thirty-five to fifty years, 39¾ per cent. from sixteen years (the youngest) to middle age, 24 per cent. in persons from fifty to sixty years. As far as these statistics go, then, it would seem that 76 per cent. occur before fifty years, and only 24 per cent. after this time. In the collection of cases, 122 in all, in 5 of which no age is given, 59 per cent. were below sixty years of age, 41 per cent. above.

Occupation plays a very important *rôle* in the production of heart symptoms with obesity; a man who has been active up to thirty-five or forty, and then leads a life of idleness combined with fat-producing conditions, is apt to suffer with a fat heart. This may explain the great preponderance of the affection in males; females, as a rule, continue with their vocations or do not have occasion to change their habits from intense activity to physical idleness as often as men. The abuse of alcohol or the taking of great quantities of fluid by a fat individual, certainly predisposes to fat heart under proper conditions; in two of my own cases, fat heart has developed in the bookkeepers of large breweries. These men consumed enormous quantities of beer daily (from thirty to sixty glasses); their only occupation consisted in keeping their accounts, and when the time for exercise in the evening came they were physically unable to take it. As a result, fat accumulated, and the heart became affected. It is remarkable that, although the men who are engaged in working in breweries drink just as much beer, they are not affected by this form of heart trouble. This

alone goes to show that it takes more than obesity to produce fat heart, for brewers, as a rule, are above average weight.

As will be shown further on, all fat people are in danger of having not only their hearts surrounded but also invaded by fat. But given two very obese persons, it is a problem why one should have heart symptoms and another not. The influence of sex, occupation, and alcohol has been referred to above; everything causing heart strain or irregular activity of the heart may predispose to the further development of fat heart. In this category of causes may be enumerated excessive use of tobacco, coffee or tea, great and frequent excitement. Frequently patients with fat heart are in a condition bordering on neurasthenia, frequently their whole character becomes changed from good nature to irritability, but it seems impossible to determine whether we are dealing with cause and effect, or *vice versa*. It is not uncommon to find a man who has been obese since his twentieth or twenty-fifth year, and has carried his fat without symptoms of any sort, manifesting signs of fat heart after he has gone through prolonged trouble—the loss of a fortune in one of my cases, and the struggle to get another. The symptoms here will develop gradually and, combined with a neurasthenic condition, there will be present all the signs of fat heart.

PATHOLOGY.—Leyden has divided the disease into two classes: 1, fatty growth around the heart; 2, fatty infiltration of the heart. The latter class he subdivides into two; one without and the other with arterio-sclerosis. It has seemed unnecessary to me to make the subdivision, as we can consider arterio-sclerosis a complication, but hardly one of the prominent features of fat heart. It will be seen from the table that only 30 cases had atheromatous deposits, yet, as additional proof of the point raised, it will be remembered that this table is composed principally of cases which have been observed upon the post-mortem table as well as clinically. It has been impossible to find a very great number of cases which have recovered on record—these, manifestly, being the ones which ought to decide the question. We might, perhaps with more reason, make a subdivision of fat heart with bronchitis or dropsy, for both of these conditions, although complications, are found in nearly all well-marked cases at a certain stage of the disease.

From the standpoint of pathology, modern medicine has done very

little to add to our knowledge concerning the relation fat bears to the heart. It seems, furthermore, that it is not the pathology that interests us, especially in our present state of knowledge, as the objective signs, certainly for one state of the diseased condition, are hardly characteristic for any particular change in the heart substance. Quain states that Rokitansky in his *Handbuch d. path. Anatomie*, ed. 1842, has described three forms of fatty heart: 1, fat in excess upon the surface, 2, fat among the muscles, in excess; and 3, fatty degeneration (I have failed to find this in the edition of 1856). Sir James Paget was the first to introduce these views to the English public in his series of lectures published in the *Medical Gazette* for 1847, and the divisions made by Rokitansky are, practically, those made by Leyden as quoted before.

Of the first form, fat in excess upon and around the heart, little need be said in this connection. We find it in obese subjects, and it consists simply of an increase of fat in the connective tissue of the mediastinum, and the various layers of the pericardium. Upon the external surface of the heart the fat is found in excess wherever there is connective tissue, and this especially around the superficial bloodvessels to begin with. As a result, we have the fat increased in the sulci of the heart first, then over the ventricles; the right ventricle is affected more than the left on account of the greater number of superficial bloodvessels lying upon it. There is usually quite a large deposit of fat at the apex, and, frequently, a fringe extending along the right border of the right ventricle. A great many cases will be found in which the heart is so buried in fat that none of the muscular tissue can be seen. In these cases, however, it is doubtful whether we are dealing with a pure case of fat heart of the first degree, as the condition of the myocardium decides this question.

As far as can be seen from the incomplete record, no microscopical examinations having been made, it seems that some cases are recorded in which the myocardium remained perfectly healthy, although invested by fat. From a pathological standpoint we are dealing, as a rule, with a case of the second class, when the fat around the heart becomes developed to such an enormous quantity as to invade the myocardium. The line of demarcation between a normal and an abnormal deposit is one that cannot be drawn with mathematical accuracy. In all the cases investigated by modern methods, it seems to be abnormal when

it interferes with the functions of the myocardium. Again, this is a point which cannot in all instances be determined with certainty by post-mortem investigation, thus giving additional force to Leyden's argument for the acceptance of the condition as a clinical and not a pathological entity. Yet the decision of pathological increase of fat cannot be difficult for the experienced pathologist in the majority of cases. The essential part of the change consists in the intrusion of fat upon the muscular substance of the heart; the results are atrophy of the muscular fibres and fatty degeneration

The first change is the one seen most commonly; it is due to pressure by fat, and, as a rule, is associated with a "distortion" of the muscular fibres. It is remarkable how little fatty degeneration is found in these cases, a fact to which Quain already calls attention, and Leyden insists upon with great force. The latter author goes so far as to state that wherever fatty degeneration of muscle is found, it is due to general and not to local causes. It would be remarkable to admit that the anæmia which must follow the development of fat in such quantities around the bloodvessels could not produce fatty degeneration; but opposed to this is the fact that fatty degeneration was found to a very limited degree in the five cases examined in this direction by Leyden, one of which was also investigated by Grawitz. More extended observation is desirable, especially as to the relation of those parts degenerated and the quantity of fat around the bloodvessels supplying those parts. Upon macroscopic examination, the myocardium will be found normal in size in some instances, but always less resistant and usually softened, easily torn, and of a pale brownish color. Cases have been described in which the fat has given its own color to the muscular substance. The fat makes its inroad from without, so that we see the muscles under the pericardium invaded most, and those under the endocardium least, the fat invasion extending through the whole substance in very severe cases. The papillary muscles would naturally be attacked, on account of their connection with the outer layers of muscular fibres, but this does not seem to be as frequent as was once supposed.

The mechanism of the changes that follow is a very simple matter. The substitution of fat for the hardest and most resistant muscle in the body leads to dilatation, best marked where there is most fat; sometimes hypertrophy follows, sometimes there is myocarditis. Rupture

of the heart takes place where the process has lasted for a long time, and where it is well developed. In the cases collected, this took place in 25 out of 122; most frequently upon the left side of the heart (in 18), upon the right side in 3 cases, once in the ventricular, once in the auricular septum. In 2 cases locality is not mentioned. It is a remarkable fact that valvular lesions are quite uncommon, for in the whole number of cases collected only 16 had lesions of the valves, which were, for the most part, due to processes of a different nature. As the result of circulatory troubles, we see the usual changes which take place in a great many different forms of heart disease. The lungs are congested, their bloodvessels dilated, and their whole volume sometimes increased. In all probability this is is due to the stasis which occurs in them, whether the circulation be disturbed from the right or the left side of the heart.

SYMPTOMS.—The first class of cases presents few or no symptoms that could be called characteristic. The patient, like all fat people, is short of wind, cannot stoop often without losing his breath; whilst walking or running will produce dyspnœa. The same can be said for great or prolonged excitement. Physical examination may or may not reveal anything abnormal. If the patient be very fat, percussion will give only an approximate idea of the size of the heart, otherwise, the area of heart dulness is found increased on account of fat upon the pericardium. Palpation frequently reveals a diffuse heart impulse, but it is normal just as frequently, although it may be feeble. Auscultation simply shows normal heart sounds. The pulse in these cases is strong, neither rapid nor slow. If an analysis of these symptoms be made, we will find that we must base our diagnosis upon attacks of dyspnœa and enlargement of the heart dulness in a fat person. The first symptom can be explained without taking the heart into consideration as cause; the breathing space is smaller than normal, on account of accumulation of fat within the thorax and the abdomen, and this is especially the case when the patient bends over. Furthermore, if we look to the heart, fat people have more blood, absolutely, than lean ones (Recklinghausen, Oertel); therefore, any slight exertion will give the heart in a fat subject more work than it can do with comfort, the size of the heart not being increased with general obesity. The second symptom, enlargement of heart dulness, may, in individual cases, be well enough developed, but it seems to me very risky to base

a diagnosis upon simple enlargement of the heart dulness in a fat patient, not to say anything of the difficulties experienced in detecting this enlargement. At best, then, it seems justifiable only, to suspect fat growth around the heart.

In the second class of cases we have a clinical picture which is more distinctive. The patient comes to the physician and complains of symptoms that are easily referred to some affection of the heart. He has been fat for years, and has been bearing his fat perfectly well until a comparatively recent time, when he complains of one or another symptom which leaves no doubt as to where the cause of trouble lies. The least exertion causes dyspnœa; he will be waked up at night, especially after a full meal, by an attack of dyspnœa, or he finds it difficult to get to sleep, after having retired, on account of violent and painful palpitation. Or he has more or less well-defined attacks of angina pectoris, at one time represented by violent pains in one or both arms, without dyspnœa, at another, attended by the most pronounced difficulty in breathing. Again, the complaint may be that the patient is suddenly attacked by asthma, of cardiac nature, or that he has cardiac asthma almost constantly. In some cases, short attacks of a semicomatose or apoplectic character are complained of. Rhexis of bloodvessels accompanied by true apoplectic seizures takes place when atheromatous processes are associated with fat heart. It will be seen that there is nothing absolutely characteristic in these symptoms, except that they are the result of weak heart or of degenerate bloodvessels.

As far as the subjective signs are concerned, it is not necessary to point out here that fat in the myocardium will not produce symptoms differing from those of the heart weakened or dilated from other causes. Neither does it seem a fit place to add that a dilated or weak heart will be followed by the same symptoms, as far as circulatory disturbances go, whether the cause be myocarditis, aneurism of the heart, or fatty overgrowth. At best, we must base our diagnosis upon a combination of signs, as it is impossible to depend upon the subjective symptoms when the diagnosis can be made. The subjective symptoms vary enormously, as can be best illustrated by the two following cases:

CASE I.—Y. Z., male, aged thirty-seven years; weight 227 pounds; size five feet eleven inches. Family history good. This man had been an actor from his twenty-second to his thirty-fourth year of life. Before that, studied

at a German university. Within the last three years has been bookkeeper and partner in a large liquor store; states that he does not drink much daily, but occasionally has to drink too much. Has had all the diseases of childhood; muscular rheumatism after Franco-Prussian War, in which he was an officer; had gonorrhœa, had never had syphilis. During the time of his activity as an actor he never weighed more than 165 pounds, until 1881, when he began to gain, so that in 1883 he weighed 230 pounds. He was able, by means of exercise, to reduce himself, but never lower than 200 pounds. Since he has changed his occupation he has gained steadily, and now complains of restless nights, and of sleeplessness during the day and night. Occasionally he has pain in both arms, especially in the left one, which sometimes becomes very acute, radiating from the region of the heart into the shoulder and from there to the hand; but not accompanied by dyspnœa. When he stoops or eats a full meal the blood rushes to his head and he feels very much oppressed over the chest. His eating has so much effect upon his breathing that, latterly, in order to satisfy his appetite, which, he claims, is not very good, he has been forced to eat more frequently than before, but much less at a time. Has palpitation of the heart when he lies down, and cannot walk or run as he formerly did without bringing on an attack of palpitation and violent dyspnœa. In these attacks he says that his chest feels as if it would burst, and could not on account of being surrounded by a hoop of iron. Examination reveals heart impulse diffuse and weak in the left mammary line; heart dulness extending to one inch beyond the right of the sternum, to the mammary line on the left and to the level of the second rib upon the sternum. Heart sounds clear but muffled. Pulse ranging from 90 to 100, weak and easily compressible, but perfectly regular.

This is a specimen of the beginning of trouble. The case following is one of a much severer type. (In narrating both these cases, much has been omitted which has not seemed essential.)

CASE II.—X. Y., male, aged forty years, weight 190 pounds, size five feet eight inches. Father died of apoplexy, mother of diabetes mellitus. Consulted me May 29, 1887. States he has been stout since his twenty-fifth year. Had left him a large fortune by his father, which he lost on Wall Street under circumstances involving much litigation, and leaving him a complete bankrupt. This occurred several years before he noticed anything of his present ailment, except that he had became very nervous. He ascribed this to worry and anxiety, and to his continued want of success in being able to provide for his family. Drinks moderately now, but has drank too much. Smokes excessively—fifteen to twenty cigars a day. Has had gonorrhœa, chills and fever, erysipelas (1875), and is a hay fever subject. In September of 1886 he first noticed the beginning of his present illness. This consisted of a spasmodic contraction about the chest, accompanied by dyspnœa when he walked fast. The attacks became more violent, the pain extending especially to the left arm, and coming on without the provocation of exercise.

Thinking about anything interesting, or exciting at all, will provoke an attack. At first these attacks lasted but a few minutes, but now they last much longer. On May 2, 1887, had an attack coming on at night, asthma, associated with pain in the head and chest, which left as suddenly as it came. The next night he was kept awake all night by a similar attack, which left him sore the next morning. He consulted a physician, who prescribed a solution of nitroglycerine, which seemed partially to control these attacks. Upon examination, the apex beat was found outside of the mammary line, dulness upon percussion began here, but did not extend to the right of the sternum, nor higher than normal. Auscultation showed a systolic bruit at the apex, very soft, a change in the accent of the first and second sound, and an accentuation of the second pulmonary tone. The pulse was small, but not too fast. This patient was treated, sent to Marienbad eventually because he could not be completely controlled at home, and upon his return the following note was made: The heart reveals nothing abnormal except a slight indistinctness of the first sound of the apex. Accentuation of second pulmonary sound disappeared. Patient has lost over thirty pounds in weight, and feels perfectly well.

Upon examining the heart, we are enabled in individual cases to arrive at a positive conclusion; this is even admitted by Rosenbach (Ziemssen's *Real Encyclopœdie*, vol. vi. p. 484), who, in general, denies the possibility of a positive diagnosis. As has been pointed out before, percussion in a very fat individual does not give satisfactory results; we can say, in general terms, that the area of dulness is increased as it was in the first form, sometimes more definitely to one or the other side. In reëxamining some of my cases, I have availed myself of auscultatory percussion, and find that, even in very fat people, the heart dulness presents a decidedly different outline from the ordinary when examined in this way. My experience in this direction, however, has been so very limited that I would not dare to say anything about the absolute value of this method.

By palpation, the heart impulse is found to be very diffuse, and, in some instances, it cannot be felt at all, it matters not in what position the patient be placed, whether lying down, standing up, or leaning forward. Auscultation reveals something abnormal in every case. At the apex the first sound is changed in one way or another, almost the first manifestation being that the trochæic rhythm is altered. The first sound becomes weaker and less accentuated, and, in some cases, has disappeared entirely (Hardy, *Bull. de l'Acad. de Médecine*, 1882, p. 236).' It has also been stated that the first sound has been redupli-

cated, so that the valvular and the muscular sound could be distinguished, the one from the other. A low bruit is sometimes heard with the first sound, which has been attributed by authors to different causes. Curschmann (*Deutsches Archiv f. klin. med.*, 1874, p. 93) describes a case of fatty infiltration of the heart, in which the papillary muscles were infiltrated as well as the rest of the heart; in this case a bruit was heard toward the end of the illness, with the first sound at the apex, no accentuation of the second sound at the pulmonary, so that Curschman calls the bruit hæmic. On the other hand, Cratz (*Wiener med. Wochenschrift*, 1864) relates a case in which there was decided infiltration of the papillary muscle, yet no bruit was heard. In all the other cases which I have looked into, no bruit was present when the papillary muscles were affected, although it would seem that, theoretically, a bruit could be produced by degeneration of the papillary muscle, which is also admitted by Schroetter (Ziemssen's *Handbuch*, vol. i. p. 236), yet, practically, this does not occur. The cause for this is probably to be found in the fact that a sufficient number of muscular fibres are left to prevent the mitral or tricuspid valves from becoming insufficient. The bruit is not of a valvular origin, because, as has been pointed out before, valvular lesions are very rare in fat heart; and, secondly, the other physical lesions do not correspond with a valvular lesion. The bruit which is heard must, I take it, be due either to the irregular contraction of the muscular fibres of the heart, or it must be hæmic in its origin. It would lead too far to discuss the question, but it seems to me to be the same bruit which is heard in those cases of chorea in which we are able to establish neither the existence of a heart lesion nor anæmia.

An accentuation of the second pulmonary sound is sometimes heard at the base of the heart, especially in those cases in which the left side of the heart is dilated and weakened; but this is by no means of sufficient frequency to lay stress upon. Yet its presence can be of importance from a diagnostic standpoint. In the second case I have reported, there were present all the physical evidences of a mitral regurgitation, yet if percussion could be relied upon, and this seemed to be the case, as the patient was not too fat, there was enlargement of the left side of the heart, and none in the right, which naturally precluded the diagnosis of mitral regurgitation. Thus, then, and it is unnecessary to adduce other illustrations, there may be an incongruity

of physical evidences which alone leads to the proper diagnosis in a fat person. The pulse differs from the weak and rapid to the weak nd slow, irregular, or intermittent, or the latter combined with rapidity. It is almost characteristic of the condition to have a large, apparently healthy and robust individual present himself with the pulse of a child. If the sphygmograph be used, it will be found that the tracings vary enormously. Kisch divides the cases into four groups: 36¼ per cent. have a pulsus tardus which is sometimes anacrotic, in 32 per cent. the pulse is subdicrotic or dicrotic, in 24 per cent. there is increase of arterial tension (arterio-sclerosis), and in 7¾ per cent. there is arhythmia, from an intermittent to an irregular tracing. A slight amount of exercise brings out the true character of the pulse, sometimes changing the tracing so that it is hardly recognizable as coming from the same patient.

Examination of the other organs may or may not reveal anything abnormal; this depends entirely upon the heart. It has seemed to me, however, that an examination of the urine was necessary in every case, for two reasons: it gives us a knowledge of the degree of heart affection (albuminuria existing or not); and, secondly, we get an idea of the metabolism of the body (which we can get by no single method of examination, and which is invaluable as far as treatment is concerned) from a careful, quantitative as well as qualitative urinalysis. Examination of the urine is of the greatest importance.

Diagnosis.—A great deal has been said and written on the subject of diagnosis of fat heart, from absolute negation of its possibility in any case, to positive enthusiasm of its recognition in every instance. From all that has been said, it will be seen that the fact that there is a change in the myocardium is the only diagnosis we are justified in arriving at. If we now take into consideration that we are dealing with a fat person, the chances are that the case is one of fatty overgrowth of the heart. But we are not justified in excluding degenerative changes in the myocardium—these are frequently found in the same individual. A decision may be arrived at in the individual case if we weigh all the symptoms carefully; and, again, it may be almost impossible to make a differential diagnosis between fatty overgrowth and a valvular lesion. Latham (*Lectures*, XXVI., p. 328, New Sydenham Society, 1876) has expressed our position very aptly: "we are

able, during life, to conjecture a fat heart with such strength of probability that we almost know it."

PROGNOSIS depends very much upon the form that we are dealing with. Cases of the first class are to be looked after, because they will eventually develop into the more serious second form. It is a problem still to be solved, why some fat people escape fat heart, and why others who are by no means so fat will suffer. It is within the observation of every one that persons may be enormously heavy, and yet have no heart symptoms whatsoever. It is impossible to determine what percentage of fat people suffer from heart complication. Kisch, in the introduction of his book on "Die Fettleibigkeit" (1888), says that he has seen over 10,000 cases of obesity in an experience of twenty-five years at Marienbad. Further on (page 91) he speaks of having in latter years utilized sphygmographic tracings of 700 obese persons "in whom the diagnosis of fatty overgrowth of the heart could be made with probability bordering on certainty." The phrase "in den letzten Jahren," in late years, causes one to conclude that he has seen more than 700 cases of fatty heart, so that there is no certainty in utilizing his figures. This much can be stated with certainty, that any obese person is in danger of fatty overgrowth of the heart, and that the weight does not form a positive index as to the chances of its occurrence. For the individual, the amount of involvement of the heart must be the principal factor as to prognostication, although the age, general condition, and form of obesity must not be lost sight of.

TREATMENT.—The treatment of fat heart is the same as that of obesity. In the majority of instances we remove all that there is of the disease when we remove the fat; the muscles of the heart resume their natural functions, and the patient is relieved. There are a great many ways of reducing a patient's weight (I presume it is a part of modern medical science to call *these* methods, and by various names). There is the Banting (Harvey), the Ebstein, the Oertel, the Schweninger method, and several others. The principles upon which they are based are identical, absolute reduction of food taken in twenty-four hours, and plenty of exercise. To this, Oertel and Schweninger add reduction in quantity of fluids introduced. Ebstein prefers the fats to the starchy and saccharine foods, but allows mixed diet like all the others, with the exception of Banting (Harvey), who prescribes an almost exclusively albuminous diet. As there is no doubt that all of these

methods have succeeded, so there can be no doubt that the principles upon which they are based are correct. It is necessary to add, only, that there is an indication for each one or none of these methods; in other words, that every case must be treated as an individual one, and that the so-called methods can give us nothing more than general outlines, which must be filled up.

The treatment by saline cathartics is a very valuable adjunct—these may be in the form of mineral waters or medicaments, to be used, however, in suitable cases. The two latest works which have been written (Oertel and Kisch) take opposite sides on the question of benefit from Marienbad, Carlsbad, etc. The true state of affairs will probably be found in the middle. Oertel, who lays most stress upon the collection of fluid in the system, and the disturbance of hydrostatic equilibrium, warns physicians at bathing resorts not to accept these patients; yet the action of saline cathartics is to withdraw fluids from the system. Kisch, who finds a fine difference between Marienbad and Carlsbad, to the enormous advantage of Marienbad, advises the use of sweat-baths which withdraw water. It would be folly to send an anæmic obese patient to Marienbad, just as it would be extremely risky to put a patient with thin and dilated heart-walls through Oertel's plan.

A great many remedies have also been recommended, some with specific properties, some without them; some of them are very useful, such as arsenic, iron, but none will do the work without the proper diet. One thing seems to be agreed upon by all observers: that whatever treatment is used, must be continued for some time. This will be illustrated by the following case, of which I give the outlines.

CASE III.—*March* 2, 1879. A. R., aged fifty-five, male, weight 298 pounds; size five feet ten inches. Comes on account of a heavy cold which he says he has contracted. Has been coughing for several years. Has been a saloon-keeper for ten years; before that, unsuccessful merchant. Began to gain weight after changing occupation. Drinks not less than twenty, sometimes as high as forty glasses of beer in twenty four hours (from one and a half to three gallons). Takes more or less wine and whiskey. Rarely goes out of his saloon, takes no exercise whatsoever. Finds it impossible to get to his sleeping apartment on the second floor without resting three or four times on account of difficulty in breathing and palpitation. Cannot walk any distance without stopping to recover his breath. Becomes dizzy when he stoops, etc. Examination shows enlargement of right side of the heart, no heart impulse,

first sound at apex very weak, accentuation of second pulmonary tones and evidences of a general bronchial catarrh. Pulse very weak, rapid, and regular. Urine contains traces of albumin.

Treatment consists in reduction of fluid taken into the system, exercise, and Carlsbad salts. It was not necessary to order diet, because the man ate very little. The patient gradually reduced the quantity of liquid he was taking to three small glasses of cognac a day, and as he did this his appetite began to improve, so that a diet list was made for him, quite generous, mixed, and limited as to quantity only. He began by walking a given distance without resting (one-half square); then was told to increase this, and to do this continually, so that after two weeks he was able, without fatigue, to walk twenty times as far as when he first started. In the course of this time he had lost fifteen pounds, and all his symptoms were improving. He was then told to climb slight elevations, of which there are a great many in Cincinnati, and after three months of treatment he climbed some quite high hills which surround the city. After six months of treatment he had reduced himself to 245 pounds (loss of 53 pounds), and was feeling perfectly well. He had to give up his saloon for financial reasons, started in another occupation, and did well. For two years (until 1881) he kept up with his treatment, occasionally gaining in weight a little, and then making extraordinary efforts to reduce himself, in which he always succeeded.

About this time his new venture began to fail him, he grew careless, began to drink again, and gradually regained almost all that he had lost (weighing 285 pounds). On July 17th he was attacked with an apoplectic seizure, coma set in, and in a few days he died. A post-mortem was not permitted.

Oertel's method, briefly stated, consists in the exercising of the heart, reducing the quantity of fluid in the system, and a diet. Exercise of the heart is accomplished by causing the patient to climb hills of various grades, the distances being marked off and under the control of a physician. By means of this exercise the following is accomplished: strengthening of the myocardium and increase of its function, since this muscle behaves like any other muscle of the body; secondly, an increase of blood to the right side of the heart; thirdly, a flushing of the pulmonary circulation; and, lastly, increase of blood in the left side of the heart and in the systemic circulation under normal circumstances. In order to accomplish all this without danger to the patient, it is necessary to pre-suppose certain pre-existing conditions. The first thing to be taken into consideration is that the arteries are in healthy condition, so that the resistance to the emptying of blood from the left side of the heart is not great enough to cause damage, such as producing dilatation of the left ventricle, etc. When atheroma is present, therefore, Oertel's plan cannot be utilized.

Secondly, before beginning the method, it is necessary to determine, if possible, what the condition of the myocardium is. Our limitation of knowledge comes in here—it is impossible to say positively what we are dealing with—hypertrophy of the heart, with or without dilatation, dilatation alone, myocarditic changes, or what not. In this, then, comes the risk of using the method. Up to date, not a single case has been reported in which harm has been done by the plan. From theoretical considerations, a great many cases ought to exist; they have not occurred, so that the future must decide whether or no the danger is as great as we suppose. Every case of fat heart (extensive atheromatous cases excepted) ought to be treated with this method, or some modification of it (Bad Nauheim). The following argument is conclusive to me: if nothing be done, in bad cases, the patient will be lost, and if this method be used, he can be saved (Oertel says all cases without atheroma). The first trials are to be made under the supervision of the physician, and it is astonishing how soon the heart, and with it the general circulation (dilatation of the arteries and increase in pressure), becomes accustomed to the additional labor. In the discussion at Wiesbaden, Lichtheim laid especial stress upon the danger of producing stretching (Dehnung) of the ventricular wall. Yet it must be remembered that increased resistance is aimed at in this treatment of fatty overgrowth, as well as an increase of intracardiac pressure. This is accomplished by strengthening of the myocardium, by exercise, and the bringing together of the muscular fibres by means of the removal of fat. That the method has done this has been proven by several cases.

The dietetic treatment which Oertel uses belongs to the subject of treatment of obesity, in which we are only indirectly interested. As for the hydrodynamic treatment of myopathic heart trouble in general, as proposed by Oertel, it applies with great force to some cases of fat heart. Although it has been proven by Schroetter and Lichtheim that withdrawal of water does not increase the density of the blood very much (2–3.4), and although Bamberger has shown that fat people do not suffer from serous plethora, yet the abstinence or withdrawal of water in some fat people is a principle not to be overlooked. Whatever the effect upon the system may be, which is disputable ground, reduction in the quantity of fluid is sufficient to cause loss of weight in a great many fat people. As far, then, as fat heart is concerned, Oertel's plan is the best in suitable cases; it has its limitations, it

requires great care, but until some definite charges beyond theoretical objections are brought up against it, it should be tried in all those cases in which no resistance is offered on the part of the arteries or the lungs.

The medicinal treatment is the same as that of any other trouble of the myocardium (digitalis, nitroglycerine, strophanthus, etc.). I might add that, in my limited experience, morphia is badly borne by these patients and that a great deal can be accomplished by valerian or some of its preparations.

Total number of cases reported:

	Cases.
Males	88
Females	34
Total	122

Age.	Cases.	Per cent.
Below 10 years	0	0
10–20 "	1	.85
20–30 "	6	5.12
30–40 "	14	11.96+
40–50 "	19	16.23+
50–60 "	29	24.78+
60–70 "	36	30.77+
Over 70 "	12	10.24+
Total	117	

Occupation:

	Active.	Retired.
Males	22	77

Atheroma of arteries:

	Cases.
Aorta	21
Coronary	14
Other arteries	4
Total	39

Rupture of heart:

Left side	18
Right side	3
Ventricular septum	1
Auricular septum	1
Locality not stated	1
Rupture near aorta	1
Total	25

Division of superior vena cava	1
Lesion in valves	16

DISCUSSION.

DR. A. JACOBI: The author has spoken of fatty heart in connection with general obesity only. I am not so certain that obesity of the heart muscle and its fatty degeneration can always be thoroughly separated from each other. I do not know whether that form of fatty heart described by the author can occur only in such conditions of general obesity. I think that if we take into consideration other cases of fatty degeneration of the heart in which there is no obesity at all, or only a certain amount of it, we may come to a better understanding of the nature of the condition. Both adiposity and fatty degeneration of the heart are to be taken as symptoms of malnutrition. They may be the result of an excess of fats and carbons, or of a congenital condition. I have a case in mind which I will narrate: A lady, aged thirty-two, had been anæmic all her life, being rather fat. She died with all the symptoms of pernicious anæmia. In her case there was fatty degeneration of the heart, and it was very slightly, if at all, enlarged. The cause of the fatty degeneration was malnutrition, which was due to the smallness of all the arteries of the body. The aorta and all the large arteries were very much smaller than normal, but in other respects they were normal. I would say that this condition resulted in insufficient nutrition and consequent insufficient muscular power of the heart, and finally in fatty degeneration. The heart muscle undergoes the same changes that every other muscle does. When all the arteries of the body are too small the arteries supplying the heart muscles are also too small. We have to look for fatty degeneration of the heart when we have to deal, not only with a surplus, but with a want of nutriment.

DR. F. P. KINNICUTT: Professor von Esmarch has very recently informed me that he had seen a number of cases treated under the personal supervision of Oertel, with exceptional success. I believe that Oertel has somewhat modified his original views.

DR. F. C. SHATTUCK: These cases are not as common in this country as in Germany, and not as common in my section of this country as in parts where there are more Germans. My practical experience with such cases has been very small. In reference to the results of Oertel's method of treatment, my remembrance of the report of the discussion which took place at the Wiesbaden Congress is that damage had resulted, under the observation of several of the speakers, which was of a serious nature. It seems to me there is no question of serious dangers in the Oertel plan of treatment, but these dangers are connected with the difficulty of diagnosis. It seems to me that disastrous results have followed Oertel's method where it was applied to cases to which it did not belong. Therefore, especially at the beginning of treatment, great caution should be exercised.

DR. JAMES TYSON: In regard to the diagnosis of this condition of fatty degeneration, it occurs to me that true fatty degeneration is not associated with obesity, as is fatty infiltration, but is rather the result of chronic endocardial disease, chronic Bright's disease, or some one of the acute infectious diseases. So that the diagnosis from this standpoint resolves itself into this: given a case of obesity with the symptoms of weak heart and the absence of any one of the conditions named, either acute or chronic, we must come to the conclusion that we have a fatty infiltration, or an obese heart, which might be safely treated by Oertel's method.

DR. H. C. WOOD: I think that the Oertel plan of treatment, when applied to a certain class of cases, is of great use; but the application of it is always attended with danger if we have a case in which the cardiac symptoms are severe, and are dependent upon a true fatty degeneration of the heart muscle. In the *London Medical Record* there are reported cases of grave injury by the Oertel plan of treatment. The theory of the Oertel method is that the heart is a muscle and must be strengthened by exercise, like any other muscle that has been wasted by want of use. But the heart never suffers loss in this way, because it must always perform its function. A heart in which the muscular fibres are seriously involved may give out under strain, and the symptoms become intensified by exertion. The great danger of the method lies largely in the impossibility of diagnosticating how far the morbid process has gone in the particular case. The treatment should only be used under very careful supervision, and with the distinct understanding, on the part of the patient, that there is a certain amount of risk in it.

DR. F. P. KINNICUTT: Oertel himself is very far from merely prescribing free mountain-climbing. On the contrary, he graduates the distances to be walked very carefully, even to the distance in feet. His study of the individual patient is minute. He prescribes the diet and the times of rest; in brief, he takes every element of the case into account.

DR. H. C. WOOD: I know that the Oertel method calls for a withdrawal of fluids from the diet, stopping the use of alcohol, and the giving of a light diet free from fats; but its peculiar feature is the exercise, and that is liable to be overdone, or carelessly employed.

DR. FORCHHEIMER: The method of Oertel must be defended against the conception of it which seems to exist. It does not consist in taking a given patient to the top of a high mountain; nor does it, in its beginning, mean great fatigue or great risk to the patient. It is a gradual, graded, and progressive method of strengthening the heart; beginning with small demands upon that organ and ending with greater ones. The exercise of the heart is carried on under the careful supervision of the physician, and the tasks laid out for the patient are such as have been empirically determined as proper for that patient. The diminished quantity of fluid which the patient is allowed to take is of the utmost importance. It seems to be pretty conclusively proven that, in

certain cases, this alone is sufficient to cause loss of weight in obese subjects. However this may be, it seems rational that, as the blood takes up the fluid from the alimentary canal, the less fluid is introduced the less does the quantity of blood increase, therefore, the less work will have to be performed by the heart. The experiments quoted in the paper are not conclusive as to the effect a greater or less quantity of fluid would have upon the heart's work—in order that this can be decided it is necessary to study the effect of the ingestion of fluid upon the blood in those vessels leading to the heart. As to damage having followed the use of Oertel's method, I would refer to what has been said in the paper, again emphasizing the fact that, up to the present, not a single case has been published in which the patient had been injured; and some cases have been published in which, if this method had not been used, the patient would have died most certainly. The contraindications are those put down in the paper,—the same that Oertel himself had agreed to in his last publication, and they are the logical outcome of a study of the physiology of the method. No doubt can exist that the so-called method will be abused and possibly has already been abused.

It is not the province of the discussion to take up its applicability to other forms of heart trouble, but as far as fatty overgrowth of the heart goes, it seems, at present, to be the only one which can promise complete restoration, or at least such a restitution that, functionally, the heart is in excellent condition.

CAUSAL THERAPEUTICS IN THE INFECTIOUS DISEASES.

A Clinical Experiment.

By J. C. WILSON, M.D.,
ATTENDING PHYSICIAN TO THE PHILADELPHIA HOSPITAL AND TO THE JEFFERSON MEDICAL COLLEGE HOSPITAL.

Enteric Fever—Phthisis.

The discoveries of Hoffa, Brieger, Vaughan, Bourget, and others within the last few years have done much to clear up the relation between the special pathogenic germs of the infectious diseases and their symptomatology. They have also rendered necessary a new definition of the infectious diseases. In the words of Vaughan and Novy,[1] "An infectious disease arises when a specific, pathogenic microörganism, having gained admittance to the body, and having found the conditions favorable, grows and multiplies, and in so doing elaborates a chemical poison which induces its characteristic effects."

The germ theory as a working hypothesis has revolutionized surgery and robbed midwifery of the chief of its terrors. It has given us an antiseptic procedure which reaches the perfection of simplicity in ideal cleanliness and recently boiled water. It has made quarantine reasonable, preventive medicine a power, and prophylactic measures intelligible to the people. Isolation and disinfection have now an obvious and definite purpose. But until it came to be known that pathogenic microörganisms produced the definite chemical poisons called ptomaines the demonstrated facts of bacteriology shed but little light upon the clinical phenomena of the infectious diseases; still less did they directly influence treatment except locally. For countless investigations have shown that antiseptic medication by way of the mouth has

[1] Ptomaines and Leucomaines, 1888

little effect upon the specific bacteria of a developed disease. Nevertheless, despite innumerable failures, something has already been accomplished, and, to quote Welch,[1] "One need not be of a very sanguine temperament to hope that our steadily increasing knowledge will bear fruit, not only, as in the past, in the prevention of these diseases, but also in a rational system of causal therapeutics."

The production of special ptomaines by specific bacteria is probably a necessity of the growth and multiplication of these bacteria, and that which interferes with the splitting up of the complex chemical compounds of the body proportionately interferes with that growth and multiplication. It is in this way, doubtless, that antiseptics render organic substances insusceptible to the action of putrefactive germs rather than by direct action upon the germs themselves.

What may be the mode of action of vaccination or of the inoculation of attenuated virus is at present past finding out, but the effects are manifested upon the tissues and the fluids of the body. It is by acting upon the substances that constitute the culture media of the bacteria that their growth and multiplication are to be interfered with by therapeutic means, rather than by directly acting upon the bacteria themselves when they have already invaded the organism and caused disease.

The cases which form the basis of this communication are submitted to your consideration as illustrations of a mode of treatment not hitherto employed in enteric fever and pulmonary tuberculosis. I have no theory to offer to the Association as to whether or not the salts of mercury undergo possible modifications when taken by the mouth, which they escape when administered hypodermatically, nor as to possible differences in their effects when undergoing slow absorption and acting continuously, as compared with their action when given in separate doses in a form permitting rapid absorption and elimination.

The cases are submitted as a contribution to the subject of causal or etiological therapeutics in contradistinction to all forms of symptomatic and expectant treatment.

Rendot,[2] in 1887, treated with satisfactory results a series of cases of enteric fever by the administration of mercuric chloride in daily doses of from 2 to 5 milligrammes ($\frac{1}{33}$ to $\frac{1}{12}$ grain). This quantity

[1] Modes of Infection. Annual Address, 1887.

[2] Gaz. Hebdom. Sciences Méd., Bordeaux, December 11, 1887.

was given in divided doses at intervals of two hours. To the objection that in these minute doses the drug is inadequate to produce physiological effects, Dr. Rendot replies that the doses are scientifically rational, and that they fulfil a positive indication of the typhoid infection. They are insufficient to destroy the vitality of the bacteria of typhoid fever, but possess, he believes, the power of neutralizing the toxic principles produced by their functional activity. Is it not possible that the action of the mercuric chloride tends rather to restrain the formation of these toxic principles?

Some months ago while investigating the method of treating syphilis by the intra-muscular injection of calomel,[1] as first suggested in 1864 by Scarenzio and revived in 1885 by Neisser, it occurred to me that this plan might prove valuable in other forms of infection. It has certain definite advantages over the ordinary methods of medication, among which are infrequent administration, precision of dosage, and promptness of action. As the calomel thus introduced into the organism undergoes gradual conversion into the albuminate of mercury, and in the presence of the alkaline fluids of the tissues into a double chloride of mercury and sodium, this method has the further advantage of exposing the organism to the continuous action of mercury in small amounts. It, moreover, possesses the cardinal advantages of sparing the digestive tract all direct irritation, and the certainty of absorption without regard to the state of the alimentary canal.

The calomel is suspended in a mixture of glycerin and water, and injected deeply into the gluteal or deltoid muscles under rigid antiseptic precautions. The operation is usually followed by transient pain, and by circumscribed irritation and deep induration; if properly performed, abscesses do not follow. The induration usually disappears in the course of a week. The dose of calomel injected is one to two grains; it is repeated every four or five days. I have no experience with the red oxide of mercury used in this way, nor with the "gray oil" of Lang.

Enteric Fever.

CASE I.—E. G., male, æt. thirty-one, unmarried, Italian seaman, was admitted to the medical ward of the Jefferson Medical College Hospital on the 1st of November, 1887, on the eighth day of an attack of enteric fever,

[1] J. C. Wilson, M.D., "Note on the Treatment of Syphilis by the Hypodermatic Injection of Calomel," Medical News, June 2, 1888.

contracted while aboard ship in the Delaware River, opposite Philadelphia. The evening temperature on three occasions after his admission to the ward reached 105° F.; the morning temperature range was about 103° F. The temperature fell to normal on the twenty-first day of the attack, and, with the exception of occasional transient febrile exacerbations, remained at the normal until the fortieth day. During this time, however, it was noted that the area of splenic dulness continued to be enlarged. On the fortieth day a relapse occurred, with recurrence of the rash and diarrhœa, and the febrile movement reached 103° F. This relapse was complicated by the occurrence of a large abscess in the left inguinal region. Defervescence was complete on the fifty-ninth day from the beginning of the primary attack and the nineteenth day from the beginning of the relapse. From this time convalescence was uninterrupted. Hypodermatic injections of calomel in grain doses were employed in this case throughout, the administration of the dose being on several occasions followed by a distinct and permanent lowering of the temperature range.[1]

CASE II.—H. O., æt. twenty-one, an acrobat by profession, was admitted to Ward 8, Bed 5, of the Philadelphia Hospital, on the 19th of December, 1887, on the seventh day of an attack of enteric fever. In this case the evening temperature ranged between 103° and 104° F. There was splenic enlargement, no tympany, scanty eruption, copious diarrhœa, amounting from five to eight passages daily. The morning temperature reached the normal on the twentieth day of the attack, and the defervescence was complete on the twenty-third day. (See Chart I.)

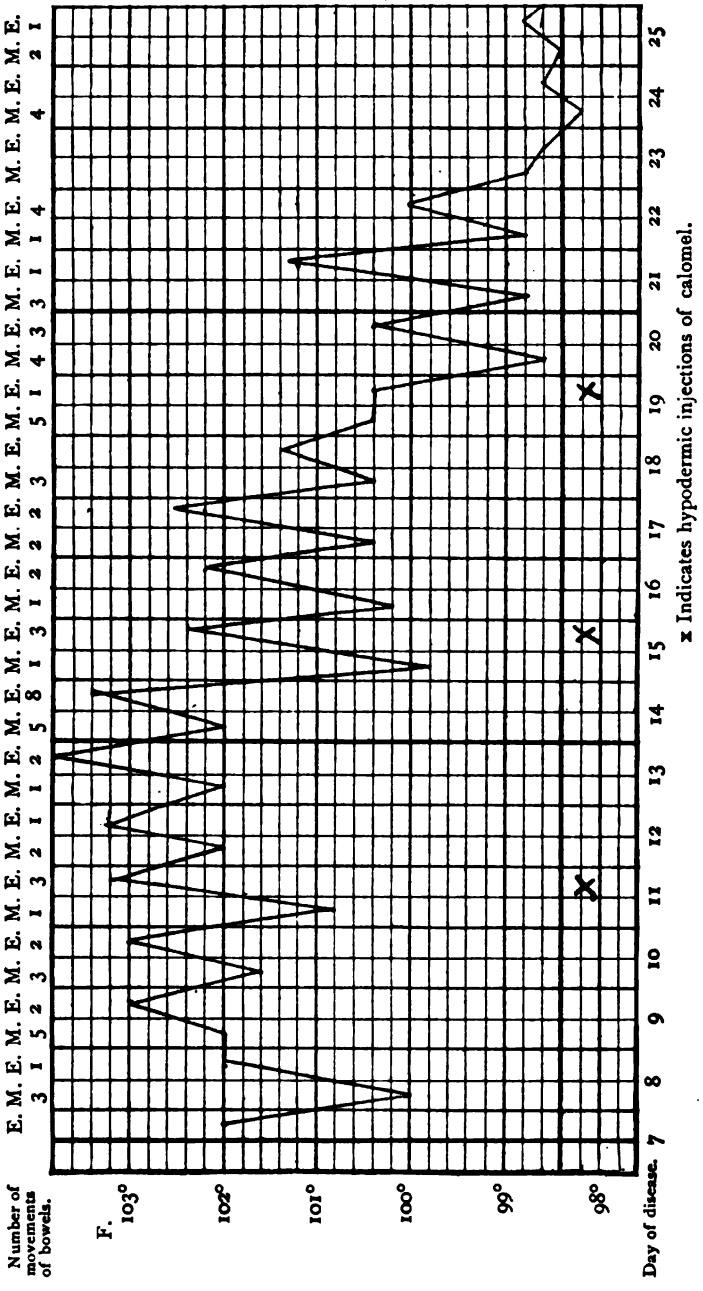
CASE III.—J. C., male, æt. twenty-four, born in Ireland, a laborer, unmarried, had been for a long time an inmate of Ward 8, Bed 6, in the Philadelphia Hospital, suffering from pulmonary consumption affecting the upper lobes of both lungs, with cavities. While absent from the hospital on leave in November, 1887, he contracted enteric fever. On the fifth day the evening temperature reached 104° F., and on the eighth day the rash appeared. The case was characterized by the intensity of the abdominal symptoms, splenic enlargement, tympany, moderate diarrhœa, and scanty but well-defined eruption. The temperature fell to normal on the twenty-fifth day and to subnormal ranges on the twenty-eighth, when collapse occurred, with an axillary temperature of 94.5° F. After marked oscillations, which continued for several days, the temperature range became that of the man's original condition. This case was treated throughout with calomel injections, and ran a remarkably favorable course.

CASE IV.—D. H., æt. nineteen, laborer, admitted to Ward 2, Bed 30, of the Philadelphia Hospital, on the 23d of January, 1888, on the tenth day of a well-characterized attack of enteric fever that had been ushered in with a distinct chill. The temperature on the evening of admission was 104° F.; the following morning, 102.4° F. The patient was delirious, tongue dry and fissured, spleen enlarged, rose spots abundant, stools thin and yellow, from

[1] The temperature charts, with the exception of those of Cases II., IV., and VIII., are not reproduced for want of space.

CHART I.

CASE II.—Enteric fever. H. O., male, æt. 21.

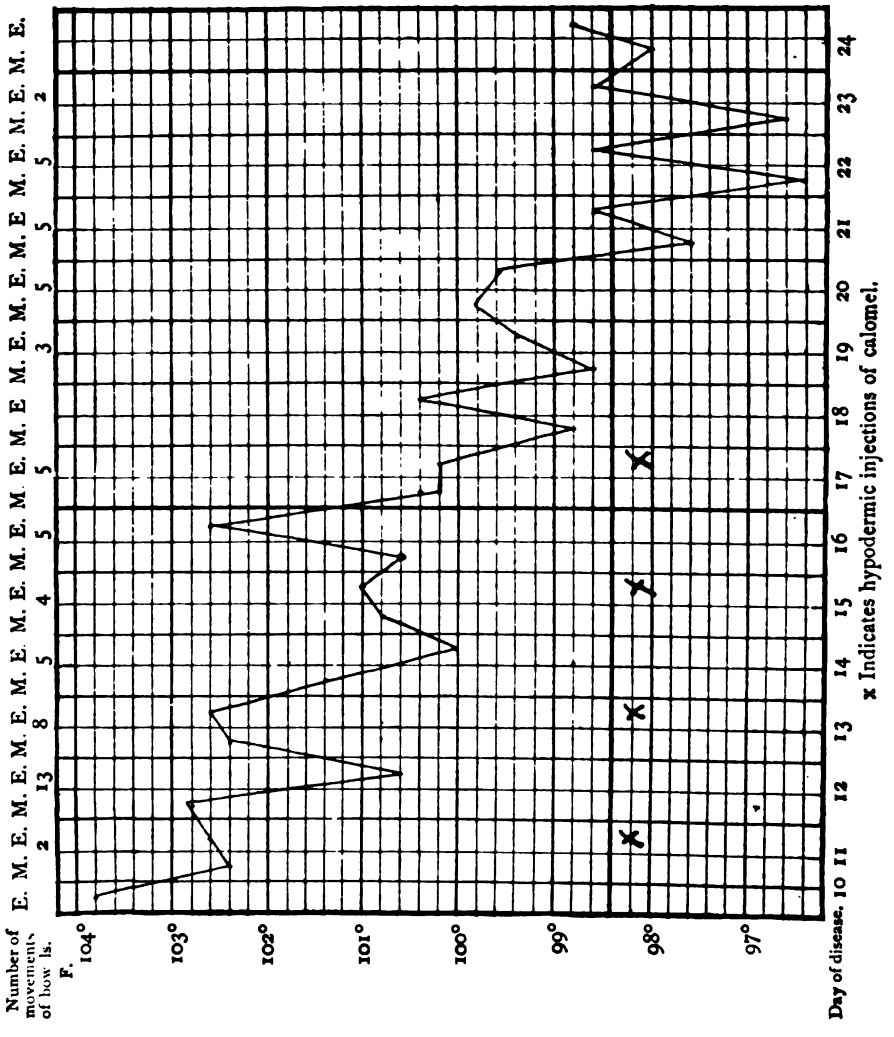

Number of movements of bowels.
E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E.
3 1 5 2 3 2 1 3 2 1 1 2 5 8 1 3 1 2 2 2 3 5 1 4 3 3 1 1 4 4 2 1
F.
103°
102°
101°
100°
99°
98°
7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25
Day of disease.
x Indicates hypodermic injections of calomel.

two to five in the course of twenty-four hours, slight epistaxis, moderate tympany.

The patient received four intramuscular injections of one grain of calomel each in the course of eight days following admission. Defervescence was complete on the twenty-first day of the attack, the form of lysis presenting a distinct modification of the usual type. (See Chart II.)

CHART II.

CASE IV.—Enteric fever. D. H., male, æt. 19.


Number of movements of bowels.
E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E. M. E.
2 13 8 5 4 5 5 3 5 5 5 2
F.
104°
103°
102°
101°
100°
99°
98°
97°
Day of disease. 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24
x Indicates hypodermic injections of calomel.

CASE V.—W. N., male, æt. fifty, unmarried, a native of Canada, mechanic, was admitted to Ward 2, Bed 13, of the Philadelphia Hospital, on the 6th of January, 1888, suffering from high fever, attended by muttering delirium, with vomiting, constipation, and great prostration. No history could at that time be obtained. The febrile movement, with the exception of a period extending from the 10th to the 18th of February, continued until the 23d of March. The case proved to be one of enteric fever of a very severe type. It was subsequently learned that the sickness began two weeks prior to the patient's admission to the hospital, and was, therefore, of a whole duration, including the afebrile period already mentioned, of about ninety days. It consisted, as shown by an analysis of the temperature chart, of a primary attack, terminating about the 20th of January, succeeded immediately by a relapse, lasting until the 10th of February, and a second relapse, which set in upon the 18th of February. During the greater part of this time the patient was very delirious. A few characteristic rose spots appeared upon the abdomen during each relapse. On the 2d of February intestinal hemorrhage of moderate amount occurred. This patient, from the time of the recognition of the nature of his sickness, was treated by calomel hypodermatically. He made a complete recovery, and left the hospital at the end of May.

It is scarcely necessary to say that the foregoing cases of enteric fever were managed in accordance with accepted rules as regards diet, bathing, quietude, and the like. Cold compresses were employed, and repeated five grain doses of antipyrin were administered in Case V. when the temperature rose above 104° F. The other cases received no medication other than the hypodermatic injections of calomel, except an occasional opium suppository, or dose of bismuth subnitrate, for the control of too frequent diarrhœa, and in one or two instances only a single dose of antipyrin.

Phthisis in Syphilitic Subjects.

CASE VI.—Widow, æt. thirty-eight; ill three years with teasing cough, attended by scanty expectoration, occasional night-sweats, slight loss of flesh, appetite fairly good, evening temperature normal. The history renders it more than probable that the patient suffered from syphilis shortly after her marriage, twelve years ago.

Physical examination revealed extensive dulness over the right lung, anteriorly in the mammary line, with abundant crepitant and subcrepitant râles heard in the infraclavicular area, and extending down the anterior border of the lung. Percussion elsewhere yields good resonance, and especially is this noted in the supraclavicular regions on both sides. Examination of the sputa showed no bacilli.

This patient came under observation the 10th of February, 1888. On the 29th of the same month treatment by hypodermatic injections of calomel was

commenced, and continued until the end of April, eight injections in all being administered. The area of percussion dulness was then noted to be much less extensive, subcrepitant râles were not heard, and a few crepitant râles only could be detected on forced inspiration after efforts at coughing. The patient left for her home much improved in all respects.[1]

CASE VII.—C. G., æt. thirty-five, unmarried, an Englishman, by occupation a painter, was admitted to Ward 2, Bed 25, of the Philadelphia Hospital, on the 8th of May, 1888. Syphilis, with well-characterized secondary manifestations, eight years ago; cough and moderate loss of flesh for the past six months; five weeks ago slight blood-spitting, severe pain in the left side, diarrhœa, night-sweats, great shortness of breath. He gives an account of having been tapped three times in the left axillary region, with the removal of bloody fluid. Patient emaciated, extremely sallow, not jaundiced, moderate cough, scanty muco-purulent expectoration, containing numerous bacilli.

The physical signs indicated moderate tubercular infiltration of both apices, with a small cavity on the left side opposite the second intercostal space. Extensive coarse friction sound is heard over the base of the right lung, anteriorly and posteriorly, whereas, in the corresponding position on the left side, there is retraction of the chest and restricted respiratory movement without friction sounds. The night-sweats at the time of admission were profuse and distressing, and the general condition of the patient wretched. The morning temperature ranged about 100° F., and the evening temperature from 102° to 103° F.; the patient weak and depressed, unable to leave his bed.

Treatment by hypodermatic injections of calomel was commenced on the 11th of May, and steadily continued, at intervals of four or five days, until the 26th of June, when the patient left the hospital of his own accord, having gained in weight and strength to such an extent that he regarded himself as a well man. The following note is taken from the hospital record:

"Great improvement, subjectively and objectively, no cough or expectoration whatever, no night-sweats; for the past fortnight the temperature range has been normal. At the right apex, anteriorly and posteriorly, there are crepitant and subcrepitant râles in abundance. At the right base pleural friction sounds are no longer heard, although the patient occasionally complains of sharp pains in this region on deep inspiration. The localized tympanitic percussion resonance in the second intercostal space to the left of the sternum, as previously noted, has disappeared. The dulness on the left side continues very marked below the clavicle, less so over the mammary region, but increases almost to flatness at the base. On the left side there is faint bronchial respiration, without râles anteriorly, and at the apex posteriorly. Tongue clean; patient looks well, has a good appetite, and weighs one hundred and forty pounds. The finger-nails show a series of well-marked transverse ridges."

[1] This is Case IX. of the series in the "Note on the Treatment of Syphilis" in the Medical News. It was thought to be a case of gummatous infiltration of the lung. The subsequent history is unknown.

This patient had no other treatment whatever except a few doses of picrotoxine for the relief of night-sweats.

Phthisis.

CASE VIII.—J. E., æt. twenty-five, unmarried, Irish, iron-moulder, was admitted to Ward 2, Bed 27, of the Pennsylvania Hospital, on the 18th of May, 1888. His illness began seven months before, after exposure, with cough and morning expectoration; has had blood-spitting in small amounts, suffered recently from chest pains, and has lost a great deal of flesh; no history of syphilis. On admission the patient was pale, much emaciated, and very weak. Cough annoying, especially at night, and accompanied by moderate muco-purulent expectoration, which contains numerous tubercle-bacilli. The chest is well formed and capacious, with the exception of localized retraction at both apices. On the right side there are the signs of large cavities in the upper lobe, with extensive infiltration of the middle and lower lobes. On the left side there are dulness, bronchial respiration over the upper lobe, and coarse crepitant and subcrepitant râles in the mammary region.

Calomel injections were begun on the 22d of May, and repeated at intervals of four or five days, two grains being the usual dose. On the 11th of June it is noted that the patient feels much improved, has gained considerable flesh, weighs one hundred and ten pounds; appetite good; bowels regularly moved each day; cough has entirely disappeared, except before breakfast. At this time there is slight expectoration, which contains tubercle bacilli; pulmonary signs unchanged.

Treatment was continued until the 30th of June, when, the patient complaining bitterly of the local pain of the injections, they were stopped. At this time the temperature was practically normal. In the course of eight days the evening temperature rose to 100° F., and in a short time reached 102° F. The calomel injections were then resumed, with the result of speedily bringing the temperature range to the normal. (See Chart III.)

It must be added though, without much bearing upon the case, that this patient, during some part of the treatment, took, at his own request, teaspoonful doses of cod-liver oil three times daily.

I have thus systematically treated in accordance with this plan five cases of enteric fever, all of which were severe, and all of which recovered, three of them running an exceptionally favorable course; and three cases of phthisis, in two of which there was a distinct history of syphilis. In the three chest cases marked and rapid amelioration of the constitutional symptoms took place without very great change in the physical signs of local pulmonary trouble. In one of the two cases in which tubercle-bacilli were found, the cough and expectora-

Case VIII.—Phthisis. J. E., male, æt. 25.

CHART III.

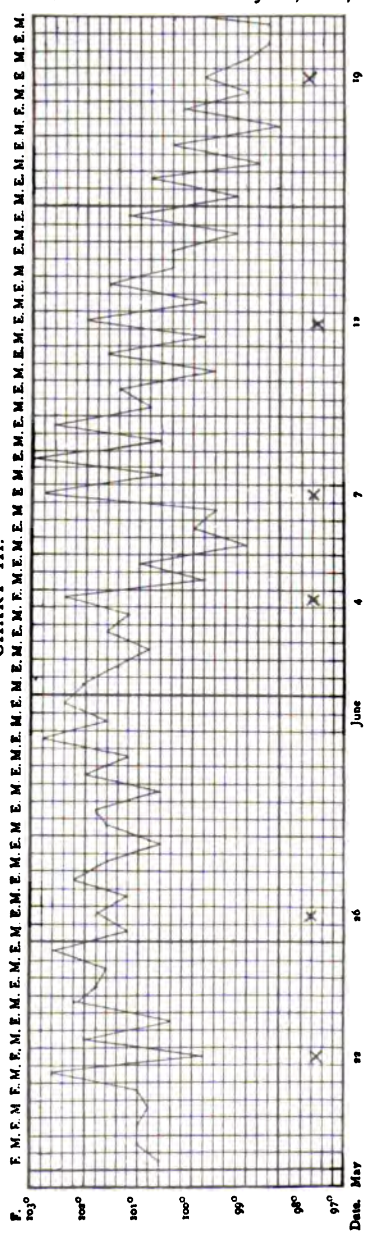
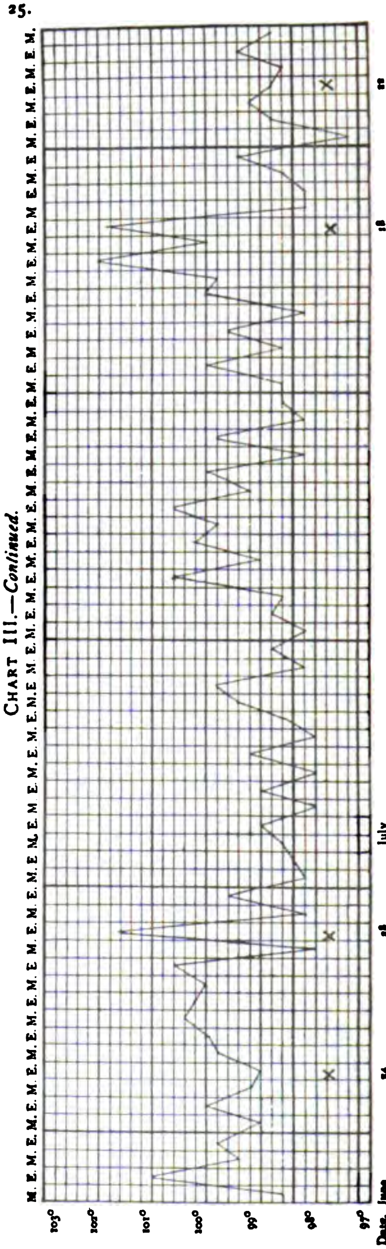


Chart III.—*Continued.*



x Indicates hypodermic injections of calomel.

tion wholly disappeared. In the other case, the bacilli persisted in the expectoration.

These cases, taken together, constitute a clinical experiment, the result of which, considered without prejudice, has forced upon me the conviction that calomel thus introduced into the organism exerts a decided therapeutic influence in ameliorating the symptoms, and in modifying the temperature range of enteric fever and of pulmonary phthisis. In the case of enteric fever with relapses (Case V.) it is impossible to ascribe any very great benefit to the treatment. Nevertheless, those who saw the case in the wards of the hospital were of the opinion that the injections of calomel were on several occasions followed by distinct amelioration of the symptoms.

DISCUSSION.

DR. GEORGE M. STERNBERG: This subject is a very prominent one to-day, and every experiment has its value. I wish to refer to a method of treatment of yellow fever which I suggested when in Havana recently. This is based upon the view that the specific infectious agent in yellow fever is present in the alimentary canal, and the fact that in this disease the urinary secretion and the vomited matters are very acid. The formula suggested was ten grammes of the bicarbonate of sodium to the litre of water, with two or three centigrammes of the bichloride of mercury. This amount was to be given in divided doses during the twenty-four hours, and continued during the first stage of the disease. I have received reports of thirteen typical cases of yellow fever successfully treated in this way in one hospital in Havana. A series of thirteen cases without a single death certainly speaks very strongly in favor of this method of treatment. I am anxiously awaiting further reports on the subject.

With reference to the cases of typhoid fever just reported, I think we need a still more extended series of cases. Dr. Wilson spoke in one of his cases of constipation and an elevation of temperature at that point. Perhaps it has not occurred generally to physicians that there is a cause for that, independent of the specific agent. There is in the alimentary canal of man a micro-organism which generates a fever-producing ptomaine. I have made experiments with this bacillus—the colon bacillus of Escherich. Two or three cubic centimetres of a pure culture injected beneath the skin or into the abdominal cavity, in rabbits, produce a rise of temperature which may amount to three or four degrees Fahrenheit. Sterilized cultures in larger quantities produce the same result. So we have a fever-producing organism constantly present in our intestines, and it is possible that in specific fevers the temperature may de-

pend to some extent upon this microörganism, which is normally present in the intestine. From the researches made by Emmerich in Naples with regard to his cholera bacillus, which is normally present in the intestines of healthy persons, and from recent researches which I made in Havana, I have come to the conclusion that this bacillus does not always confine itself to the large intestine where it belongs, but is found in the small intestine and stomach as well. I simply wish to direct attention to the fact that constipation in these infectious diseases may give rise to increased temperature as a result of the absorption of this ptomaine, and the indication is to keep the bowels cleared out.

DR. I. T. DANA: The accumulation of evidence has been such as to concentrate the attention of the profession upon the fact that infectious diseases are of bacterial origin. I have faith to believe that remedies will be found which, being introduced into the system, will destroy the disease-germs. The difficulty has been to find remedies capable of destroying these germs without, at the same time, destroying or seriously injuring the individual. It seems to me that the suggestions of this interesting paper and discussion are full of hope. Relief may come not only in the line of microbe-destruction; the tissues may be so modified as to become able to resist bacterial influence; they may be rendered non-putrefactive; ptomaines may be destroyed, or their formation prevented. For the production of an infectious disease, the two requisite conditions being a specific germ and a congenial soil, the therapeutical hope of the profession now eagerly looks for the discovery of safe means for the protective modification of one, and the destruction of the other, wherever found, whether inside or on the outside of the human body.

DR. GEORGE L. PEABODY: At the meeting last year I reported a number of cases of typhoid fever treated with naphthaline, and said that, in a certain small number of those cases, I had seen a distinct abortion of the disease. My total number of aborted cases was only three, but they were distinct abortions of distinct typhoid fever. The three cases in the first ten or fifteen were the only cases in which an abortion of the fever was secured. I am quite convinced, as the result of more prolonged experience with it, that naphthaline, as a means of aborting typhoid fever, is of very little value. I am at present pursuing the study of intestinal antisepsis in this disease, but my results are not sufficiently conclusive to justify me in bringing them before the Association.

DR. A. JACOBI: In this connection I would refer to the treatment of diphtheria with the bichloride of mercury. This disease cannot be treated by any other single remedy as well as by bichloride. It has the advantage of being readily soluble, and, in sufficient doses, to counteract the specific poison. As far as naphthaline is concerned, I have never seen a case of typhoid fever aborted, but I am positive that I have seen the diarrhœa get better, and the odor of the movements, as well as their character, change in a very short time after

its administration. In the diarrhœa of tubercular cases, also, in a large number of cases I have seen good results follow the use of naphthaline, and I am positive it is well worthy of trial.

DR. W. W. JOHNSTON: It is necessary that we should recognize the fact that in many cases of typhoid fever the temperature pursues a downward course, and that we must prove that the remedy materially alters the natural course of the disease.

DR. J. C. WILSON: I do not claim that this treatment is at present anything more than a clinical investigation in the direction of general research to discover some method of treatment directed against the causes of infectious diseases. The prevailing method of treatment is no treatment at all. Symptomatic treatment has no effect upon the general course of the disease, nor does it in any way shorten it, and a treatment of expectancy is a confession of our inability to do any more. We are not at all aware of the mode of action of two confessedly curative substances used in the treatment of infectious diseases, viz., quinine in malarial disease, and mercury in syphilis. I was led to make these clinical investigations in consequence of my very successful use of hypodermatic injections of calomel in cases of syphilis which had resisted all other methods of treatment. I have been fortunate enough to escape the production of abscesses in a large number of cases treated in this way. Cases of enteric fever treated after this plan ran a milder course than any I had treated by other methods. My object in presenting this paper to the Association was rather suggestive than instructive.

THE DIAGNOSTIC SIGNIFICANCE OF THE MITRAL PRESYSTOLIC MURMUR.

By FRANK DONALDSON, M.D.,
CLINICAL PROFESSOR OF DISEASES OF CHEST AND THROAT, UNIVERSITY OF MARYLAND.

THE characteristics of the typical mitral presystolic murmur are well known and ought to be easily recognized. It is a loud, long sound, rough and vibrating in quality, commonly with a well-marked thrill. It is audible over a fixed and limited position—the mitral area—a circle of about an inch around the point where the apex touches the thoracic wall; the direction of the sound being toward the apex. Its maximal intensity is to the right of the apex beyond where the blood current enters the ventricle. It may be heard indistinctly above the third rib and, very exceptionally, in other directions. Its rhythm is at the end of the long pause immediately preceding and abruptly ending at, and with, the first sound. The rhythm of the presystolic murmur, when the pulsations are slow, is easily recognized by feeling the carotid pulse while auscultating, because the pause before the first sound is much longer than that which follows it. But when the beats are more rapid the distinction cannot be drawn, for the increased pace is gained at the expense of the period of rest, and one pause may be as short as the other (Fagge). The designation presystolic is sufficiently appropriate, since the murmur immediately precedes the ventricular systole. These important facts show that it is found during the diastole of the ventricle. The closure of the sigmoid valves, the filling of the ventricles, and the contraction of the auricle, take place during this period of the heart's revolution. With the murmur we often hear pronounced accentuation of the first sound at the apex, and of the second sound over the pulmonary artery, and, also, when it has existed for some time, a reduplication of the second sound.

The blood in the lesser circulation rushes on through the distribution of the pulmonary arteries into the elastic pulmonary veins, thence directly onward through the auricle into the ventricle, continuing its flow until the ventricle, as well as the auricle, is nearly full, when suddenly the auricle contracts on its contents and completes the distention of the ventricle previous to its systole.

Ludwig and Hess showed that the mechanism of the closure of the left auricular-ventricular orifice does not reside in the valve alone; the surrounding muscles of the ventricle have an active share not merely in floating up the valve curtains, but in reducing the size of the aperture which these valves have to close. The papillary muscles keep the curtains tight during the contraction of the ventricle. The whole action of the heart is screw-like, six or seven distinct layers of muscular fibres crossing one another. The base muscles do their share of the work of closure, the valves promptly complete it. When the muscles of the base are enfeebled, the valve curtains are insufficient to close the orifice (MacAlister).

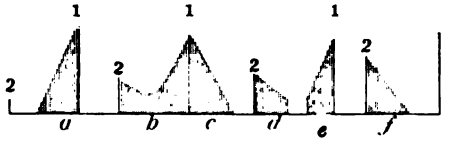
It is a mistake to suppose that the presystolic murmur is always the same. Usually it is the most prolonged of cardiac bruits. It may be very short; so short, indeed, as to be with difficulty separable from the natural first sound. Occasionally it appears like a tone or accentuation of the first sound. In this case the first sound becomes so sharp and clear that it may easily be mistaken for the second sound, which becomes inaudible at the heart's apex, and, unless careful attention be paid to the rhythm, there is danger that the murmur may be mistaken for the normal sound, instead of an abbreviated presystolic bruit.

It is a very variable sound both as to its length, its loudness, and its quality. It is heard at some times and not at others. It may, and indeed often does, follow immediately the second sound, and continue up to the culmination of the first sound, with which it suddenly ceases. It is generally more intense at the ending than at the beginning. Traube says that, if the action of the heart be retarded by digitalis, this murmur ends before the first sound. Flint states that in only one of his cases did the murmur cease before reaching the first sound. It often commences in the middle of the diastole with the auricular systole, and runs up to the first sound. Again, it lessens in volume at the middle, and, rising, ascends to its first intensity.

It sometimes becomes altogether inaudible in the middle of the long pause, and then reappears. In some cases there is an entire absence of a murmur, but instead we hear an accentuated clicking first sound.

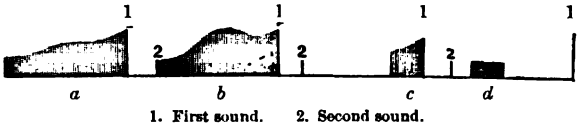
The name of post-diastolic is given, very inappropriately, to the murmur heard immediately following the closure of the sigmoid valves. This sound, though rough, is not blubbering and vibrating. It is generally indicative of pronounced stenosis. The diastole of the ventricle begins with the closure of the sigmoid valves. A bruit that immediately follows the second sound, which, it is admitted, is found at this precise period of the cardiac rhythm, ought to be designated diastolic without prefix of "post." These two murmurs, the presystolic and post-diastolic, may be present independently of one another, and when found in the same patient may coalesce, or they may be separated by an appreciable interval.

FIG. 1.



Typical and other varieties of presystolic and post-diastolic murmurs.
1. First sound. 2. Second sound.
a. Presystolic murmur (typical). *b*. Presystolic murmur united with post-diastolic.
c. Systolic (mitral insufficiency). *d*. Post-diastolic, separated from presystolic.
e. Presystolic *f*. Post-diastolic.
(After Sansom.)

FIG. 2.



1. First sound. 2. Second sound.
a and *b*. Continuous post-diastolic and presystolic murmurs.
c and *d*. Irregular presystolic.
(After Gairdner.)

Although much has been written of late about the presystolic murmur, yet all who have had much experience in consultations in cardiac cases will confirm the statement of Broadbent, that it is not infrequently, even now, entirely overlooked and often mistaken for the mitral systolic murmur, the differential diagnosis of which has been clearly established. Formerly it was supposed that the presystolic

murmur was rarely found, but closer observation has shown that it is of frequent occurrence, especially among women and children. Flint observed it in sixteen out of thirty-nine cases of mitral disease. Its grave prognostic significance alone justifies the attention that has been given to it. In this paper, I shall only detain the Association by a brief study of the factors entering into the production of this interesting murmur when of organic origin.

There must be two factors in the production of "fluid veins," whether they are obstructive or regurgitant: the lesion, and the propelling force, together with the other well-known influences, modifying blood pressure. For the generation of sonorous fluid veins, we must have the passage of a jet of blood forced through a narrow constriction into a wider cavity or part of a vessel.

It is acknowledged that mitral stenosis is the lesion in a very large majority of cases in which a presystolic murmur is heard. Many prominent authorities believe that this lesion is invariable. The constriction is frequently found at post-mortems in cases in which there had been no presystolic murmur detected during life.

The mitral valves and the auricular-ventricular orifice are specially liable, in early life, to valvulitis from acute or subacute rheumatism and other causes, such as scarlet fever, measles, or chorea. These leave formations of connective tissue which narrow the orifice itself in the annular forms or by diaphragmatic partitions with buttonhole orifices, or the leaflets adhere together and produce funnel-shaped constrictions. In some instances the valves lose completely their natural formation. These new conditions are most favorable to the production of murmurs, which are due to oscillations in the blood itself. The current of blood passes through a small opening into a wider cavity, and is divided up into a number of jets, forming fluid veins. The rapidity and force of the current must cause variations in the intensity of the sounds.

The obstructive lesion of the mitral orifice, it must be borne in mind, is of very gradual formation, of months or of years. It commences often insidiously. The auricular-ventricular orifice is so large that it can bear some lessening of its size without seriously interrupting the proper filling of the left ventricle. Thus stenosis may be sufficient to produce the vibratory sonorous murmur, without any marked subjective symptoms.

While admitting that stenosis of the mitral orifice is, in a very large proportion of cases, the pathological lesion met with where this murmur is found, yet there are numerous cases reported in which the mitral orifices were not altered in size, but in which other obstructive and narrowing conditions were found inside the ventricle—such as shortened chordæ tendineæ, thickenings and rugosities about the leaflets of the valves or in the walls of the ventricle, to account for the production of the characteristic prolonged, blubbering, presystolic murmur. Wherever and whenever we find physical conditions to produce "fluid veins" with vibratory phenomena, we can have the same sound. We need not recall Flint's three cases in which aortic insufficiency, as demonstrated at post-mortems, produced functional murmurs with the characteristics of the mitral presystolic murmur. Dr. Bramwell has published such a case, and Dr. Guitéras reported three. Sansom speaks also of cases somewhat similar to Flint's. Osler has had two well-marked instances.

The writer has never clinically met with such a case, but he has heard aortic diastolic murmurs where there were marked accentuation and shortening of the first sound, similar to the effects met with in cases of mitral stenosis. While it is true that such cases have been reported by the most competent auscultators, they are in fact but rarely met with. There is, however, no *a priori* reason why they could not exist. Aortic regurgitation murmurs are diastolic in time, and if, as Flint claims, the leaflets of the mitral divide the reflow current in the same manner as in the production of the murmur at the auricular-ventricular orifice, the sounds may be found and a prolonged murmur of presystolic rhythm with the blubbering vibrations may be heard. Dr. Guitéras maintains that this functional presystolic mitral murmur is more frequently heard than Dr. Flint supposed. Balfour, Nixon and Sansom, and others think differently. There can be no doubt that the diastolic aortic murmur can be conducted to the apex by the solid walls of the hypertrophied ventricle, the current of blood itself as well as by the sternum and walls of the chest. The infrequency of these functional mitral murmurs, when there is an aortic insufficiency, is difficult to explain.

Dr. Balthazar Foster suggests an explanation of how this murmur is heard best at the apex: That different valves are affected in different cases, and that, if the left sigmoid near the mitral is affected, the

blood falls directly back into the ventricle and the bruit is heard best at the apex; whereas, if the right or posterior sigmoid valve is diseased, the regurgitant blood impinges on the septima and the bruit is carried down the right side of the heart. Dr. Guitéras maintains that these propagated murmurs are, in fact, mitral obstructive murmurs, and that they are more apt to develop when the posterior aortic segment is affected, because in such cases the recurrent stream is brought directly against the anterior leaflet of the mitral valve.

Dr. Bramwell (*Diseases of the Heart*) refers to a case of rupture of the posterior coronary segment of the aortic valves in which no presystolic murmur was audible. There must be some unusual physical condition which has not yet been appreciated.

There is also a murmur which Barlow, Leaming, Flint, and Donaldson have designated as intraventricular, which, when there is much hypertrophy of the left ventricle, seems to occupy the first portion of the systole. This resembles the qualities of the presystolic, and, when the heart is beating rapidly, may be confounded with it. Of course, in diagnosticating mitral stenosis, everything is taken into consideration, together with the murmur.

Let us inquire from what sources the necessary force of the blood-current is derived. Dr. Gairdner taught that it is produced by the systole of the left auricle, and he accordingly named the adventitious sound the "auriculo-ventricular murmur." This could not possibly apply to the sound which exists before the contraction of the auricle, which, in health, occupies one-fifth of a second in time, and immediately precedes the systole of the ventricle. The feeble force of the auricle in health was shown by Chauveau, who, by prolonged irritation, exhausted its contractility, and found that the ventricle continued to act effectively, and kept up the circulation. Ludwig confirmed this result by introducing a tube through the auricle. In fact, the auricle may be regarded as the dilated termination of the pulmonary veins, acting as a reservoir to supply the ventricle, and to regulate the blood-pressure in it. The auricular cavities are never completely obliterated during contraction, though this does not appear to be the case with the auricular appendages.

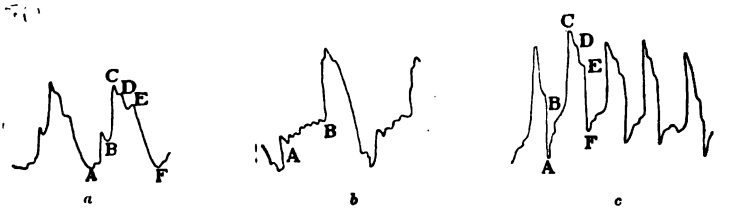
We may ascribe the production of the presystolic murmur, in beginning obstruction of the mitral orifice, to the force of the left auricle. Temporary compensation is promptly established in the force of the

circulation. It seems that there is a reserved force, as in involuntary muscles, which is called forth in the emergency.

We further admit that, as the result of obstruction at the mitral orifice, impeding the influx of blood into the left ventricle during the diastole, the left auricle, having extra work to perform, does become first dilated and subsequently hypertrophied. Indeed, percussion, in rare instances, reveals abnormal bulging over the auricle, and frequently the pulsation of the auricle is perceptible about the fourth rib. The walls of the left auricle, which are normally about one and a half lines in thickness, may be increased to as much as a quarter of an inch, with proportionate augmentation in strength. It may be that the prolongation of this bruit is due to this hypertrophy, but it is almost incredible when we know that it is the longest, as well as the most irregular and vibrating, of all cardiac murmurs. We question whether it is a force sufficient by itself to overcome the resistance caused by advanced mitral constriction, and to give us the acoustic characteristics of the presystolic murmur.

It must be borne in mind that the auricular systole does not occur until the very last portion of the diastole, and the murmur often begins after the second sound, and includes the so-called "post-diastolic." These phenomena occur previous to the auricular systole. We readily admit that its influence is shown in reinforcing the intensity of these murmurs.

FIG. 8.



a Mitral stenosis. Pulse-rate 80. (After Galabin.)

b Mitral stenosis. Long, rough, presystolic murmur. Pulse-rate 57. (After Galabin.)

c Normal curve from man. (After Landois.)

A B. Auricular systole.
B C. Ventricular systole.
D. Aortic closure.
E. Pulmonary closure.
E F. Ventricular diastole.

In mitral stenosis the auricular portion of the cardiagrams is of longer duration, and its tracing higher, because the left auricle is dilated, and contracts with force, emptying itself with difficulty; it often shows a series of wavelets, giving it a serrated appearance corre-

sponding to the purring thrill felt by the hand. Ott, Hess, and Galabin attribute the late closure of the pulmonary valves to the diminished elasticity of the artery in consequence of long-continued high pressure.

The cardiagram of the normal heart (see Landois's *Physiology*) proves that the aortic valves close a little before the pulmonic valves. They close, however, so near together that only a single sound is heard. In mitral stenosis (stage of non-compensation) the aortic valve closes sooner, and the pulmonic valves later than normal. This is because the left ventricle contains less blood, and the right more than normally. There results what is called a reduplicated second sound, which is an increase of a physiological condition. All conditions which cause the aortic valves to close rapidly (diminished amount of blood in the left ventricle), and the pulmonary valves to close later (congestion of the right ventricle)—both of which exist in mitral stenosis, favor the production of a reduplicated second sound, which occurs in one-third of the cases of mitral stenosis.

If mitral contraction occurs at a very early period of life, whether as a primary affection, or secondary to a corresponding narrowing of the tricuspid, other parts of the circulatory system become accommodated to it, and the patient may reach adult life before the lesion produces any symptoms—the post-mortem revealing the fact of the organic defect. Dr. Wilkes reports cases in which resulting hypertrophy of the right side in children was so great as to cause bulging of the chest, and in which the pulmonary artery partook of the hypertrophy, and became thicker than the aorta. On the other hand, cases occur in which, owing to mitral stenosis early in life, so small a quantity of blood is thrown into the left heart that there is imperfect nutrition of the whole body, which accommodates itself to the defect. Hypertrophy of the right ventricle and of the left auricle compensates so thoroughly for the deficiency, that there may be no subjective symptoms, and no bruit, perhaps, beyond an imperfect first sound.

Dr. Gairdner did not ascribe the production of the presystolic murmur wholly to the muscular contraction of the auricle. Dr. Wilkes held that the murmur might anticipate the auricular systole, and that it might occur not only at that time, but also during the heart's diastole and pause. Dr. Galabin came to a like conclusion from the evidence afforded by the cardiograph. Dr. Sansom claims that the causation of the presystolic murmur may be independent of the auricle;

he states "that in many cases he had observed that though there had been a prolonged presystolic murmur commencing in the long pause almost immediately after the second sound, cardiographic evidence has shown the auricular systole to occupy its normal position just anterior to the commencing contraction of the ventricle." He cites a case in which a murmur occupied at one time a portion and at another almost the whole, of the long pause, and the autopsy showed that the auricular systole could have had no share in producing such a murmur, for not only was the left auricle so dilated that its walls could have exerted no appreciable muscular power, but it was lined by a closely adherent, old, laminated blood-clot. It thus appears that the

FIG. 4.

Sansom's case, showing mitral stenosis, while auricular systole is in normal position.
A B. Auricular systole.

post-diastolic murmur, as well as the presystolic, may be caused in the diastole, and be due to the entrance of the blood independently of the auricular systole, it being urged through the stenosed aperture, owing to the tension under which it has been retained in the elastic and distended auricle and pulmonary veins.

We feel justified in concluding that the systole of the left auricle is a factor of greater or less force in the production of this mitral direct murmur, but it is not the only one, and the others may produce it independently of the auricular systole.

NEGATIVE PRESSURE.—Let us see whether any negative pressure is brought to bear upon the left auricle and pulmonary veins to assist in getting the current into the ventricle. Is there any suction action of the left ventricle of the heart? Until the publication, in 1878, of Goltz and Gaule's experiments, the majority of physiologists attached little or no importance to any suction action of the heart, although in all times there have been zealous advocates of the doctrine that diastolic suction was an important factor in the circulation. Goltz and Gaule, by the use of ingeniously constructed maximum and minimum manometers, determined the maximum and minimum pressures in the left and in the right ventricle. They found with natural respiration,

in the left ventricle of a dog, a negative pressure of 52 mm. of mercury. In the right ventricle there existed, in another case, a negative pressure of 17.2 mm. of mercury.

In accordance with the generally accepted ideas, they at first were inclined to attribute this negative pressure to the expansion of the chest during inspiration. To their great astonishment, however, they found, after the chest was opened, and independently of respiration, a negative pressure in the left ventricle of 23.5 mm. of mercury. This latter negative pressure, therefore, cannot be due to the elastic traction of the lungs. They attribute it to the elastic resilience of the ventricle following the systole, like that after compression of an elastic balloon. They say: "The adherents of the doctrine, according to which the heart acts as a suction-pump, will find their boldest expectations surpassed when they hear that the suction force of the left heart of a large dog, at the beginning of the diastole, is equal to a column of water 320 mm. (23.5 mm. of mercury), and that this force in a healthy man is probably even much higher." (Goltz und Gaule: "Ueber die Druckverhaltnisse im Innern des Herzens," *Pflüger's Archiv*, 1878, Bd. 17, S. 100.)

In 1879, Moens published an article ("Die erste Wellengipfel in dem absteigenden Schenkelder Pulscurve," *Pflüger's Archiv*, Bd. 20, S. 517, 1879) in which he attempted to show that the negative pressure found by Goltz and Gaule existed, not during the diastole, but during the systole. In order to make clear how a negative pressure might be produced at this period of the heart's action, he takes an elastic tube open at one end and connected at the other by means of a stopcock with a pressure-bottle. If the stopcock be suddenly closed while the fluid is flowing from the bottle through the tube, there is a negative pressure produced in the tube, beginning near the stopcock. Likewise, in the left ventricle, he argues that, as the blood is shot out by the ventricular contraction into the aorta, a negative pressure is produced in the ventricle. His chief inference, that the negative pressure is present during the systole and not during the diastole, was because a minimum manometer, passed down the jugular vein into the superior vena cava, failed to register negative pressure, as would be the case if the right ventricle exerted diastolic suction. He regards this experiment as proving that the negative pressure is not during the diastole, otherwise there would be a suction force exerted on the

blood in the right auricle and the vena cava superior. If the negative pressure is not during the diastole, it must be during the systole.

It will be observed that Moens does not actually demonstrate that the negative pressure is during systole. He infers it indirectly. On the other hand, he seems to have overlooked the fact that Goltz and Gaule found a negative pressure in the right auricle of 11.2 mm. mercury (this might have been due to inspiration), the negative pressure in the right ventricle, in the same case, being 17.2 mm. mercury. Moens is quoted by Martin and Donaldson, Jr., as having shown that the negative pressure exerts no suction force on the blood in the auricles.

In consequence of the doubt which Moens's work raised as to the period at which negative pressure existed in the ventricles, de Jager, in 1883 ("Ueber die Saugkraft des Herzens," *Pflüger's Archiv*, Bd. 30, S. 491), published experiments on this question, and came to conclusions in opposition to Moens's and in confirmation of Goltz and Gaule's. De Jager determined that the pressure at the beginning of the aorta never becomes negative, as it should be in case the negative pressure in the ventricle were due to the suction of the column of blood poured out from the ventricle. He also found that with the chest open, so that the action of inspiration on the aspiration of blood into the right heart was removed, the pressure in the right auricle becomes negative. He found negative pressures in the right auricle of 2 mm. to 6 mm. mercury. He does not believe that the auricles themselves are capable of exerting any suction force, and he, therefore, concludes that this negative pressure in the right auricle can be interpreted only as evidence of a suction exerted by it, which, of course, can occur only during diastole. The same conditions would, of course, obtain for the left cavities of the heart, as the experiments were made with the chest open. De Jager, therefore, concludes that a strong diastolic suction force is exerted by the ventricles.

As regards the causes of this suction force he mentions several possibilities, without coming to a decision. These possibilities are:

1. The elastic force developed during the systole, as in the compression of a rubber balloon.

2. The suction force on the exterior of the heart resulting from the negative pressure in the thorax (or the elastic traction of the lungs).

This, of course, can be only one element, as negative intra-ventricular pressure is found after the thorax is opened.

3. Shortening of the large arteries coming from the heart.

Brucke inclines, as do Goltz and Gaule, to consider these factors as the essential ones.

Gruenhagen holds that Goltz and Gaule and de Jager have established the fact of a diastolic suction action of the heart.

The experiments of Martin and Donaldson, Jr., however, on the mammalian heart, at the Johns Hopkins University, are opposed to the existence of any suction action of the heart. Their experiments are exact, and, it would seem, quite conclusive against the suction theory. It is difficult, however, at present, to reconcile them with Goltz's, Gaule's, and de Jager's experiments. They have the merit of putting the matter to a direct test, whereas the other experiments do not actually demonstrate a suction action, but leave it as an inference. They say:

"The negative pressure proved by Goltz and Gaule to occur in the ventricles for a brief period at the end of the systole, had already been shown by Moens not to affect the auricles, and, therefore, to be without effect in making the heart a suction-pump as far as the venous system was concerned.

"Once the 'aspiration of the thorax' has been eliminated, the right auricle of the mammalian heart will not receive blood unless supplied to it under a decided, if small, positive pressure. While the heart in the closed thoracic cavity may, and probably does, act as a suction-pump, this is not due directly to an active expanding force of the heart, but is the secondary result of the pneumatic conditions prevailing within the normal closed chest cavity."

If a suction action exists, it is evidently a factor not to be neglected in the study of mitral stenosis. All that increases the suction action, increases the force with which the blood passes through the narrowed mitral orifice and doubtless increases the murmur. De Jager found that when the action of the heart became feeble—as after long exposure, or by cooling, or by loss of blood—then only a slight negative pressure, or none, was produced in the ventricles. We may, therefore, infer that all circumstances which enfeeble the action of the left ventricle diminish correspondingly the force of suction, and, therefore, the murmur. Such circumstances are failure of the heart's force from

any cause, among which may be noted atrophy of the left ventricle in pure mitral stenosis. In general, whether the left ventricle is atrophied or not, the small amount of blood which it receives through the narrowed mitral orifice during non-compensation must be attended with a weaker systole, and, consequently, diminished suction and lessened murmur. All this line of argumentation, of course, is not pertinent, if we accept the doctrine that no suction force is exerted by the ventricles.

Let us further inquire whether any other force is connected with the passage of the blood through the left ventricle. We hear of the blood's passive flow through the mitral orifice. Has it no power? The right ventricle, which propels its contents with a strong wave through the pulmonary arteries and with comparative ease, owing to the absence of tonus or resistance, and of vasomotor influence to contend against, forces the blood into the pulmonary veins and through the left auricle into the ventricle. If it meets with any obstruction, the backward pressure would, of itself, add to the onward blood pressure in the pulmonary veins, although the right ventricle promptly exerts its reserved compensatory force. If the obstruction continues, the extra work of the right ventricle gradually produces permanent hypertrophy which keeps up the compensation. When this is broken, there results the disastrous backward flow of venous blood with all its unpleasant consequences upon the lungs, liver, and other internal organs. This occurs, notwithstanding the protection afforded to the process of intra-pulmonary respiration, as shown in the safety-valve construction of the tricuspid valve (King), of the pulmonary semilunar valves (Flint, Jr.), and of the obstruction safety-valve principle of the pulmonary valve (Guitéras). The increased tension of the pulmonary valves gives accentuation of the second sound. Nature further provides a relief to the lungs by the increased capacity of the right cavities by from one-tenth to one-third over the corresponding cavities of the left side.

The right ventricle becomes very gradually hypertrophied. In some cases the enormously thickened walls completely cover the left ventricle and prevent our hearing the second sound at the apex and a very feeble impulse is noticed. The left ventricle, owing to the insufficient supply of blood through the auricular-ventricular orifice, is often not only not hypertrophied, but actually materially lessened in size—

indeed, in a state of atrophy, unless there is also insufficiency of the mitral orifice as a complication. This additional lesion may cause hypertrophy of the left ventricle. May not the powerful right ventricle, by the blood pressure it causes in the pulmonary veins, be a prominent factor in causing variations in the length and intensity of the post-diastolic and the presystolic murmurs in mitral stenosis?

If this hypertrophy be marked, may it not be the propelling force producing the long sound commencing immediately after the systole and occupying the whole of the diastole? If the force be feeble, it may only produce the post-diastolic and cause the deficiency in the middle of the distance between the second and first sound. When the patient is in the recumbent position and the heart is quiet the murmur is often inaudible; on changing the position it may be heard. Active exercise increases in marked degree the intensity of the murmur. When dilatation succeeds hypertrophy of the right ventricle and tricuspid insufficiency takes place, the forces are not powerful enough to cause the murmur. The auricular systole, if dilatation has not made the walls too feeble, may give rise to the presystolic. The increased power of the hypertrophied left auricle and right ventricle certainly would cause the fluid veins to be audible vibrations in obstructive mitral disease. As the muscular power leaves the excessively enfeebled patient, the sound utterly disappears. Indeed, it is very remarkable how the murmur sometimes disappears, temporarily or permanently, the condition of valve being unaltered.

To sum up (says Dr. Galabin, in his elaborate and able paper on, "Interpretation of Cardiographic Tracings," *Guy's Hosp. Reports*) the inferences suggested by a general review of all the tracings, it appears that the evidence of the cardiograph is in favor of the view that two totally distinct murmurs may be caused by mitral contractions: first, the auriculo-systolic bruit, which may either run up to the first sound or be separated from it by a short interval; and, secondly, a diastolic bruit, due to the venous flow through the narrow and roughened orifice, which in rare cases may be blowing in quality and separated from the succeeding systole by a long pause; and that, thirdly, these two may be merged together into a compound murmur somewhat rough from its commencement, but much intensified in loudness and harshness toward its conclusion.

Marey, in some experiments by means of which he reproduced the

various sounds and murmurs of the heart, the auricle of which was not contractile, introduced a plug perforated by a hole between the auricle and ventricle, and then found that a diastolic murmur was produced, but only when the auricular pressure exceeded a certain point. Hence, he concludes that the murmur of mitral stenosis may be either diastolic or auriculo-systolic according to circumstances.

How far is our estimate of the pressure in the pulmonary veins justified by experimental research? From this source we possess but little positive knowledge. Beutner seems to be the only authority who has published any actual measurement of the blood pressure in the pulmonary veins. He found a positive pressure of 10 mm. of mercury in the pulmonary vein of a cat. In the pulmonary artery of a cat he found a pressure of 17.6 mm. mercury. These pressures were, of course, measured with the thorax open, so that we cannot attach absolute value to them. They indicate however, that, relatively to the pressure in the pulmonary artery, the pressure in the pulmonary vein is high, the pressure in the veins being more than one-half that in the artery. The pressure in the pulmonary vein, according to Beutner's measurement, is, moreover, absolutely a high one when compared with the very low pressure in the veins emptying into the right auricle. The pressure which Beutner found in the pulmonary artery is much lower than has been found by subsequent observers. The accepted belief is that the pressure in the pulmonary artery is one-third of that in the aorta; Goltz and Gaule regard it as two-fifths of the aortic pressure. If the ratio which Beutner found between the pressure in the pulmonary vein and that in the pulmonary artery holds true, we may conclude that the pressure in the pulmonary vein is even higher than his observations indicate.

Inasmuch as actual measurement of the pressure in the pulmonary vein can be made only when the thorax on one side is open, the question arises: What influence upon this pressure has the elastic resilience of the lungs under the normal conditions of respiration? If the walls of the pulmonary artery and those of the pulmonary veins were of the same thickness, then, inasmuch as both vessels were subjected to the same intra-thoracic pressure, it is evident that the changes in the intra-thoracic pressure, corresponding to the different phases of respiration, would not influence the blood pressure. The walls of the pulmonary veins, however, are thinner than those of the

pulmonary artery, therefore the aspiration of the thorax must tend to lower the pressure in the pulmonary veins more than in the pulmonary arteries. It is impossible to say how great is this influence of the elasticity of the lungs upon the pressure in the pulmonary veins.

The feeble tonus of the vessels in the pulmonary circulation is of the utmost importance, as changes in pressure in the pulmonary veins are quickly manifested in the pulmonary arteries and *vice versa*. This circumstance justifies us in assuming that change of pressure in the pulmonary artery is attended by corresponding changes of pressure in the pulmonary veins, so that changes in the force of contraction of the right heart exert a much more direct influence upon the circulation of blood in the pulmonary veins than in the left heart and the systemic veins.

Cohnheim says: "The blood in the pulmonary veins is, under physiological conditions, not like that of the systemic veins under negative pressure when it enters the heart, but is under a positive pressure which is only a little less than that of the blood in the pulmonary artery." (*Vorlesungen über allgemeine Pathologie*, Bd. 1, S. 28, Berlin, 1882.) This statement of Cohnheim does not seem to rest upon actual measurement, but to be based upon the slight resistance offered to the blood in the passage from the pulmonary artery to the pulmonary vein, and upon Beutner's measurement with open thorax. He found the pressure in the pulmonary vein more than half that in the pulmonary artery. As no means have been found which enable us to measure the pressure in the pulmonary veins under natural conditions, we are left to such inferences as the above.

DISCUSSION.

DR. J. K. THACHER: In those cases of mitral stenosis in which there is reduplication of the second sound, the aortic valves close first, and the pulmonary second. This seems to depend, at least in part, on a prolonged ventricular close-time on the right side, caused by exaggerated tension in the pulmonary artery, with, in some cases, a shortened ventricular close-time on the left side due to diminished aortic pressure.

The murmurs in mitral stenosis depend on the form and size of the auriculo-ventricular orifice and the strength of the current. The latter is determined

by the relation of the tension in the left auricle and ventricle. The fact recently set forth by Rolleston that, as seen in dogs, there is a marked depression of the tension curve of the left ventricle at the beginning of diastole, seems applicable to the explanation of the initial diastolic (so-called post-diastolic) murmur, whatever views are held concerning the suction action of the left ventricle.

DR. WILLIAM OSLER: I think that all are agreed that there is one cardiac murmur above another, which is positive and distinct in its diagnostic significance, viz., the presystolic murmur. I had an instance at the University Hospital last year, in which I demonstrated to my class a mitral presystolic murmur. The case was one of cirrhosis of the liver in a child with great enlargement of the spleen. To my astonishment, I found at the autopsy a normal auriculo-ventricular orifice. The heart was pushed up very far. I have recently written out short notes of a case of aortic insufficiency in which there was a distinct presystolic murmur. A woman was sent to my ward in the hospital in an extreme condition of emaciation. I placed my hand over the heart in the apex region and felt a very distinct diastolic thrill, and my ear also detected an extremely loud, rough murmur, which I regarded as presystolic. The woman was not in a condition to permit of further examination. At the autopsy the mitral orifice was found to be normal, but there was extreme aortic insufficiency.

DR. DONALDSON: In regard to the diagnosis between the presystolic and systolic murmur, the rhythm, character, area, and direction of the presystolic sound are different from those of the systolic. The rhythm of the presystolic alone distinguishes it from the mitral systolic. The former is heard during the diastolic period, usually at its latter portion, and terminates suddenly and abruptly with the first sound; the systolic bruit follows the first sound, is blowing in quality, without any thrill; whereas the presystolic is rough and vibrating and is accompanied, usually, with a pronounced thrill. I thank Dr. Thacher for the designation "initial diastolic," which is much more appropriate than "post-diastolic."

Dr. Osler has given us two well-authenticated cases in which the presystolic murmur existed where the post-mortems showed aortic insufficiency, but no mitral constriction. I have reported a case in which the autopsy showed both aortic incompetence and mitral stenosis, although during life there was only an aortic regurgitant murmur.

ON THE ABSOLUTE AND RELATIVE VALUE OF ALBUMEN AND CASTS, AND OF RENAL INADEQUACY, IN THE DIAGNOSIS AND PROGNOSIS OF DISEASES OF THE KIDNEY.

By ROBERT T. EDES, M.D.,
OF WASHINGTON, D. C.,
FORMERLY PROFESSOR OF MATERIA MEDICA, AND JACKSON PROFESSOR OF CLINICAL MEDICINE IN HARVARD UNIVERSITY.

I. Albumen.

Many of us can remember when the discovery and announcement of albumen in the urine were looked upon almost like a sentence of death, to be executed at an unknown but not very distant date, or, at any rate, as showing a condition of the system at which almost any medical accident might happen to the patient without further warning.

Albuminuria has held a place in medical nomenclature almost equivalent to Bright's disease until quite recently. Pathological views were not altogether at variance with these clinical ones. Bright's disease took the place of the older heart disease among the ready opinions of the coroner and policeman, and a kidney which was above reproach was among the curiosities of the post-mortem room. It was discovered however, that, whatever else albuminuria might mean, it did not point decisively and exclusively to well-marked renal lesions, but was found in many other diseases which need not be mentioned in detail, but which can largely be included in the two groups of febrile and asystolic. A step beyond this was the finding of albumen in the urine of persons in apparently good health, a part of whom, at least, continued to be so for months or years.

This is the present status, which offers several questions for examination, and this can best be made without preserving an exact historical or chronological order.

Is albumen to be found in the urine of persons in really good health? The most extensive, and among the most recent, researches bearing on this question are those of De la Celle de Chateaubourg, supplemented by the observations, some of them considerably older, of Capitan, Kleugden, Clarke, Leube, Fürbringer, Grainger Stewart, Van Noorden, and Munn; besides a considerable number of smaller groups or isolated instances, which have often the added value of having been made upon the person of the author, and carefully followed out, by Marcacci, Edlefsen, Ultzmann, Simmons, and others. According to De la Celle, albumen is to be found at times in the urine of seventy-six to one hundred per cent. of healthy persons, both young adults and children. Capitan found in soldiers forty-five and in children eighty-nine per cent.; Kleugden forty-three and a half per cent. among supposably healthy attendants in an insane asylum; and Grainger Stewart gives a general average of thirty-one per cent., which, however, covers a range of from nearly eleven per cent. among civilians to sixty-five and a half among workhouse inmates aged in the neighborhood of sixty.

The discrepancies shown by the accompanying table (Table I.) are very great, too great, in fact, to be accounted for by ordinary errors of observation or by any assumption of real differences in the classes of persons coming under the notice of the various observers; in fact, soldiers, students, and children in institutions furnish the bulk of the statistics. The discrepancies do not entirely disappear by any process of comparison of the method employed, because such a comparison cannot be accurately made for the different reagents. The figures cannot be quantitatively compared when very small amounts of albumen are in question, and even with the same method quantitative estimates of traces are only approximative at the best. The discrepancies are, however, greatly diminished by such a comparison, especially as applied to the labors of the two modern French observers who used the extremely sensitive reagent of Tanret, or the potassio-mercuric iodide, on a layer of which the urine is floated and an estimate made by the density of the cloud. Taking the figures of De la Celle, and separating the cases in which he found, by estimate, from twelve to twenty-five centigrammes to the litre, the percentage falls from seventy-seven to eighteen, from seventy-nine to twenty, from one hundred to thirty-two, and so on, as will be seen by the table. If we notice only albu-

men in the form of a flocculent precipitate, we find it in his tables only four times among one hundred and eleven soldiers who are stated to have given one hundred cases, or ninety per cent. of albuminurics.

Other groups may be examined in the same way, and it is obvious that great allowance is to be made, not for errors in observation, but for the varying criteria by which the presence of albumen is affirmed or denied, whatever be the reaction employed. Some observers distinctly state that doubtful cases are not included. It will be noticed that in the set of cases which give the highest percentages of albuminurics the maximum, if we set aside the quite small number in which flocculent albumen by heat was found, is very small—*i. e.*, from one-eightieth to one-fortieth of one per cent.; while there is a tendency to draw the line between physiological—or at least harmless—and pathological albuminuria, if such a line be possible, at a considerably higher point than this, namely, at one-tenth of one per cent.

To the delicate test (Tanret) by which the most striking results were obtained, it has been formally objected that not only does it give reactions with peptone and alkaloids, errors which may be avoided by a little further testing, but also that when the quantities are small albumen is not to be distinguished from mucin. Roberts believes a frequent source of error with the more delicate tests to be mucin.

The following conclusions may be drawn:

That the presence of albumen in minute traces, or something which gives a similar reaction with the most delicate reagents, is much more common than its absence.

That an amount of albumen detectable by heat and nitric acid may be found in a much smaller proportion of cases taken at random. This proportion varies according to the shade of opalescence which the individual observer considers conclusive, and may be approximately stated as from ten to twenty per cent. of persons examined, rising above it in certain classes to be afterward specified.

The percentage of cases of presumably healthy persons in whose urine boiling gives a flocculent precipitate is much smaller still.

As a corollary to these conclusions it may be asserted that for clinical and practical purposes Tanret's test is worse than useless. There is no reason for taking great pains to discover a sign which to the physician can have *no* significance, and to the patient, if he becomes aware of it, probably a very erroneous one, and is likely

to be the cause of groundless fears. It is chiefly in France that it has been widely used. Jaccoud says that "It is since this has become common that albumen has been so commonly found in the urine of healthy individuals."

The second question is not so easy to answer—*i. e.*, What significance is to be attached to a quantity of albumen, not large, but more than the faintest trace, occurring in a person who has no other signs of renal disease. From what has already been said, it is obvious that the number of persons in this class cannot be stated with great accuracy, but that it is not a very small one. As to the amount of albumen on the one hand which shall be considered small, we may take the limit to which many of those who have studied the subject on themselves or others, seem inclined to adopt—*i. e.*, one-tenth of one per cent. But, on the other hand, as to what amount may be absolutely neglected we cannot speak in figures, but may take the limits of the heat and nitric acid tests carefully applied. Below this point, if other signs of renal disease are not present, the trace of albumen proves nothing, and if they are, adds little or nothing to the diagnosis.

Within this class two sub-classes may be separated off with tolerable distinctness, shading, however, as most exaggerations of subnormal processes do, into each other and into the groups which cannot fairly be included within them.

First the albuminuria connected with a slight degree of anæmia, dyspepsia, headache, general depression, and disinclination to labor, bodily or mental, occurring in adolescents and often known as the albuminuria of adolescents.

Not much weight is to be attached to the remark that this is more common in the male sex, for the reason that the examinations have been made much more largely in boy's schools; in fact, the same remark may be made in regard to the statistics of other groups, soldiers, older students, and young physicians. These cases seem to have been recognized as a distinct class with well-marked symptoms for a number of years. They are certainly not to be looked upon as presenting examples of physiological albuminuria, and were, in fact, for a time thought to show a tendency to weakness of the kidneys, and to be especially liable in the future to develop a true nephritis. This suspicion has not been justified in general, but in a few cases, in which

the albumen was associated with casts, such a development has taken place. Ralfe says that, although some of these cases have a florid complexion, the degree of anæmia is sufficient to be determined by the hemacytometer. The prognosis is no more unfavorable than would be indicated by the other symptoms. The relation of albuminuria in these cases to a future nephritis is not unlike what that of a slight cough in the same patients would be in regard to a future tuberculosis; it would induce a search for other symptoms and call for attention to the general health.

The second class includes the cases of "intermittent" or "cyclic" albuminuria. In these the urine presents certain tolerably constant peculiarities beyond that of containing albumen; it is usually scanty, high colored, with a large amount of urea, and very frequently a deposit of uric acid, or oxalate of lime, the latter being the more frequent. In these cases, which are very likely to be discovered by accident, and of which naturally a large number have been found among young physicians, who have made a careful study of them, the morning urine is usually free from albumen, which appears and reaches its maximum in the forenoon, decreasing during the latter part of the day, with not infrequently a secondary rise in the evening. Exercise has a very decided effect in increasing the quantity, cold bathing perhaps even more, and digestion comparatively little or sometimes none. A few cases have, however, been noticed in which the albuminuria seemed to be determined by indigestion. The character of the food has usually but little influence. This class differs from the preceding in the absence or slightness of the accompanying general symptoms. The patients are apparently, in a large number of cases, in perfect health, though in others there may be a little general depression. The pulse is likely to be of low tension. Some cases of this kind have been followed for years without the occurrence of any symptoms leading to a suspicion of progressive renal disease. In many the albumen has disappeared without special treatment and without the change being assignable to any particular cause.

After subtracting these two classes and making some allowance for accidental cases in which there has been, perhaps, a temporary disturbance of the circulation, unusual liberties in diet, or even a slight nephritis, without alarming symptoms, which is without doubt possible, and where casts may be overlooked, we have still a number of cases

by no means insignificant, in which albumen is continuously present in the urine of persons of various ages, but beyond that of adolescence or early youth. Is this to be disregarded as of no importance, or is it to be looked upon as the early stage of nephritis? It is possible that in some of these a more minute examination, such as is by no means easy on a large scale, would bring to light casts, and settle the question of some renal disturbance, the question then becoming —What is the prognosis in slight cases of chronic nephritis?

Taking these cases, however, as they stand, and following them out carefully, as was done by Munn, we should be justified in regarding them with grave suspicion. Some of them seem to be instances of a real nephritis, running a very chronic course, and presenting at the time of their first examination no constitutional symptoms. Munn found in a considerable number of cases, in which at first only albumen was noted, that casts were subsequently present. Four of sixty-nine had died within three years, and in the majority a general deterioration of appearance was noticed.

It may further be suggested that among them there were many overweights, a class peculiarly liable to nephritis, and also that middle-aged business men, such as are the majority of those presenting themselves for life insurance, form a class especially liable to *interstitial* nephritis, a form attended with the least marked symptoms for a long time, perhaps for years.

It has undoubtedly been noticed that no allusion has been made in this paper to the *kind* of albumen found in the urine. This is not because it is a matter of indifference, but because the investigations heretofore made upon this point have been only upon a few isolated cases and are, hence, not applicable in the criticism of the statistics under consideration.

Two well-marked varieties of albumen, at least, can be distinguished, serum albumen or seralbumen, and globulin, metalbumen, or Bence Jones albumen. Beside these, hemialbumose or albumen half changed to peptone, and peptone itself are found. Mucin, although not belonging to this group, may be a source of error when very small quantities are under consideration. Any minute discussion of the chemistry of even this small group would carry us too far for our time, and at present only into the region of hypothesis; but it is by no means improbable that further experiments on the diffusibility of albumen, and

on the points of coagulability by heat, will disclose even a larger number of varieties which will have a physiological and clinical significance.

At present it appears from a small number of examinations that, when albumen in small amount is disclosed by the ordinary tests, it consists largely or even wholly of globulin, and that this has a less grave significance than serum albumen. This kind is to be detected separately from the other, by a saturated solution of sulphate of magnesia, added to the neutral urine, and the formation of a precipitate on standing. After this is filtered off the filtrate may be examined again for serum albumen.

We cannot take leave of this branch of the subject without reference to the highly important views of Semmola, according to whom albuminuria is not the early symptom, but the cause of nephritis, and finds its origin not in the anatomical changes in the kidney, but in an alteration of the serum of the blood, which alteration is in its turn the consequence of deficient action of the skin. The altered serum is, like a foreign body, eliminated through the kidneys, and on its way sets up a gradual degeneration of the renal structure. The nature of the alteration is shown by a greater diffusibility of the serum, and can only be demonstrated by physical experiments, and, as Semmola himself repeatedly points out, not by any well-marked chemical reaction.

The dyscrasic theory of Bright's disease is by no means a new one. It was held by Bright himself, and, indeed, something like it before him by Blackall. It is certainly not in disagreement with many clinical facts, but in the form advocated by Semmola it is difficult to harmonize with some of the facts we have just been considering. In the absence of clear chemical data, it is almost impossible to follow up the statements of Semmola carefully in a clinical way, but it may be remarked that his assertion that a continued subcutaneous injection of egg albumen produces renal changes obvious to the microscope, has been denied by subsequent observers. If albumen, passing for a considerable time through the kidneys, causes nephritis, the cases of innocent albuminuria, intermittent, cyclic, accidental, the albuminuria of adolescence, ought not to exist, but would inevitably lead to well-marked Bright's disease, and the question we have been discussing would never have arisen. This difficulty may be met by the state-

ment that only the altered albumen (heteralbumen) is to be looked upon as an irritating agent, and that which passes in these accidental cases, and also that accompanying renal congestion, are not of this kind, but more normal and less toxic. Such a statement is, from the reasons above given, not easy to refute absolutely; but it is, on the other hand, no more easy to prove. If nothing is the matter either with the albumen or with the kidneys in these cases, why does it keep coming through?

It seems probable that the form of albumen, so far as we can now distinguish the forms, or, to speak more correctly, so far as they are usually distinguished clinically, which has the worst prognostic significance, is that which most nearly resembles that of the blood serum. Stokvis considers pathological albumen to be serum albumen chemically. That it differs in its diffusibility is the theory of Semmola, which has not as yet the support of other experiments than his own, which are difficult to repeat in the absence of precise details of their performance.

Egg albumen has not been shown to have any special connection with the albumen of Bright's, and was arbitrarily chosen by Semmola as a representative of a foreign albumen. Whether it passes into the urine or not, as asserted by Stokvis and denied by Van Noorden, it cannot be considered to represent fairly the process taking place, according to Semmola, in the development of Bright's disease. Stokvis found that albuminous urine from a case of Bright's, injected subcutaneously into a healthy dog, produced no albuminuria, and when injected into the blood did so in only one out of several instances. This experiment is certainly more to the point as to the discharge of heteralbumen than Semmola's own, in which egg albumen was used. Hayem (*Bull. et Mém. de la Soc. Méd. des Hôp. de Paris*, March 21, 1888), though finding that egg albumen injected directly into the blood may be excreted by the kidneys, does not think that this is a fair representation of a physiological process, but that the secondary albuminuria is a result of changes produced in the blood of the recipient animal. Ascitic (albuminous) fluid could be introduced into the peritoneum of a dog in large quantity without causing albuminuria with any constancy, and when it did occur it was slight and transitory. His last experiment was more decisive than anything so far published as regards the correctness of Semmola's claims. He transfused from

a dog which had parenchymatous nephritis, as diagnosticated before, and verified after, death, a considerable quantity of blood into a healthy dog previously bled. This experiment was repeated twice with the same result, that the recipient dog had a little fever, which is usual after any transfusion, but on neither occasion albuminuria, and recovered completely.

II. Casts.

The significance of urinary casts or cylinders is a question upon which there is perhaps less necessary obscurity than in regard to albumen, but, on the other hand, less statistical information as to their presence in a state approaching health. Easy of recognition when seen, and presenting fewer doubtful appearances, the question can never arise, with a reasonable amount of experience, as to whether they have been seen or not; but as the search for them may be laborious, and the most laborious when least successful, the approximation of an unsuccessful search to a negative result depends upon the patience of the observer, and can seldom be an exact coincidence.

I can add only a few short rules to those ordinarily laid down.

Give the specimen time enough to settle, but not enough to decompose.

If a flocculent cloud appear in the middle of the liquid, look in this for casts as well as in the sediment properly speaking.

Do not use too high a power in searching for them. Find them with a low power, and examine them afterward with a high one, if necessary.

Casts are easier to class and to identify than different forms of albumen. We have first the exudative, divided into mucous, hyaline, and waxy; and the cellular, comprising pus, blood, epithelial, and degenerated epithelial. The appearance of very transparent casts, sometimes branching, irregular in diameter, and with a longitudinal striation, there is no reason to suppose has any significance, certainly no more than that of their chemical complement mucin, although it is highly probable that they are formed in the urinary tubes. These occasionally at one end appear somewhat firmer and rounded, resembling a hyaline cast, to which they seem to form a sort of transition. I have

not, however, seen that their unfavorable meaning is at all increased thereby.

I presume there can be little doubt in the mind of any one that epithelial casts indicate some form and degree of renal lesion. Normal renal epithelium is not thrown off in quantity to be recognizable, and still less in the form of casts. The significance of their appearance in regard to the diagnosis of different forms of renal disease is quite well understood. As the degeneration of the epithelium in the casts is more advanced—that is, as they become granular or fatty, so, it may fairly be inferred, is that of the kidney tubes from which they arise, except that during recovery from acute nephritis a slight degree of degeneration may affect the cells which are thrown off, while those that replace them are normal.

There is probably no form of cast more thoroughly unfavorable in its significance than the long, large, straight casts, highly granular and fatty, so as to be almost black by transmitted light, and with a very low power, or even to the naked eye, looking like fine white threads. These appear to be made up of degenerated cells welded into a more or less homogeneous mass, and are probably moulded in the straight tubes, although the material may have come from higher up. Smaller casts, coarsely granular, or with very distinct fat granules, can come from the remoter portions of the tubules, but an inspection of sections of pathological kidneys which contain casts in the cortical portion makes it seem highly probable that the most thorough degeneration may take place in the secreting tubes without the products getting into the urine in the form of casts, except as these are afterward formed by their being moulded together or enclosed in a mass of the exudation thrown out in the lower tube, and which, without the detritus floated down, would become a fibrinous or waxy cast. Some of the casts formed in the secreting tubes are undoubtedly never dislodged as such.

Returning to the exudative casts, that known as the waxy, or sometimes the amyloid (although the name is badly chosen, as tending to imply a relationship, which does not exist, with the amyloid kidney), indicates a high degree of usually chronic degeneration. I have never known but one instance of apparent recovery in which these casts were present. It was that of a young woman who entered the Boston City Hospital with "inflammation of the bowels after childbirth," and had, in addition, a copious greenish expectoration with râles, general ana-

sarca, headache, and scanty, smoky urine, with albumen and large casts, among which were many waxy. She was discharged apparently well, although still with some albumen, and much fewer casts. She was seen in the street some time afterward, apparently well, but further search has only resulted in my finding that she has not since died in Boston.

With the elimination of these forms, and the admission, which I presume few would hesitate to make, that a considerable number of casts of any kind means a distinct and more or less extensive renal lesion, the seriousness of which can be estimated by their number only in connection with other symptoms, we find ourselves in the presence of a problem not unlike that presented by a light or moderate albuminuria of the apparently healthy. Many authors speak as if the two conditions were nearly commensurate, but I believe them to be so in neither direction; that we very frequently meet with one without the other. If both mucin and mucous casts were left out of the comparison, I suspect the association would appear still less close. It is possible that a few casts may accompany a very slight amount of albumen, and, perhaps, have no more meaning than this; but it will be noticed in looking over the tables (especially Table II.) of cases of albuminuria not accompanying renal disease, that the reports of "no casts" are most frequent, and that when a statement of their presence is made it is usually in connection with the classes which border most nearly on the pathological limit.

Where hundreds of persons have been examined, the statement of "no casts" is to be received with a certain amount of allowance, but in a great many of the cases given in the second table there were small groups of persons under observation at a time, and frequently under circumstances to excite anxiety, or much scientific interest, so that we may be morally certain that many and careful microscopic examinations were made, and the statements may be accepted as being as nearly conclusive as negative ones can be.

This negative statement is made in regard to a great many of the cases of cyclic albuminuria. Where, however, the urine is distinctly irritative, *i. e.*, concentrated, and containing oxalate of lime, the presence of a few hyaline casts can hardly add to the importance of the presence of albumen. It is interesting to note that in oxalic acid poisoning (among others) albumen and casts were found in the urine by

Mürset (*Arch. f. exp. Path. und Pharm.*, xix. p. 335), and by Robert and Küssner (*Virchow's Archiv*, lxxviii.), and by von Fränkel (*Zeitschrift für klin. Med.*, 1881, Bd. 2), quoted by Mürset. In the albuminuria of adolescence, which is certainly not a physiological condition, hyaline casts have been not infrequently noted.

In old age, it is well known that the kidneys undergo a certain amount of atrophy, especially near the surface; but so long as it does not progress so rapidly that they are unable to carry off all the results of a metamorphosis, which is also, and perhaps as rapidly, decreasing, there is no reason why symptoms of any kind should arise on the part of these organs. The arterial and cardiac changes which are usually going on at the same time, are quite as likely to come into the foreground, and, perhaps, put an end to life, while the kidneys, although slowly atrophying, are still able to do all the duty likely to be required of them. Thus it comes, and very properly, that in the presence of other important symptoms referable to other organs, slight amounts of albumen and casts may be disregarded, and it is an error in diagnosis to attribute to latent Bright's disease, even when it may be actually present, everything which may happen to the patient under these circumstances.

Taking albumen and casts together, we may at first separate all the conditions of local irritation, congestion, and senile atrophy, which have before been spoken of, and in which small amounts of albumen and casts may be found. In these, neither diagnosis nor prognosis is specially affected by the combination of these two signs more than by one of them alone. If the amounts become larger, the symptoms are correspondingly important. In cases, however, in which these symptoms are the leading ones, and which cannot be brought under any of the classes already discussed, even a few casts in addition to albumen should be looked upon with suspicion, and as making the prognosis more serious. A considerable number would make renal disease highly probable, or at any rate probable enough to be taken into consideration in the treatment, and many, especially if carrying with them epithelial elements, make a diagnosis of Bright's disease almost certain.

When we have casts of epithelium, more or less degenerated, or the large homogeneous and waxy ones, we have got beyond the point at which there is room for discussion as to the presence of renal disease

of an advanced kind. Its form and acuteness are to be determined partly by the urine alone, and partly by the other symptoms which are not likely by this time to be absent.

What the nature of the lesion may be when it is clear that one exists, but when it is evident, from the length of time that the urinary signs have been present without other symptoms having arisen, that it is not a rapidly progressing one, can in many cases only be surmised.

An inflammation limited to a part of one kidney, is not a purely gratuitous assumption. The evidence of this is occasionally plain at the post-mortem. Even in the well-marked contracting kidney, the interstitial inflammation is present in patches. A small area of inflammation might for a long time furnish pathological products without extending enough to produce further symptoms.

Old renal disease may either recover or leave behind a condition which does not grow either better or worse. I have had under occasional observation a patient whose urine twenty years ago, after puerperal convulsions, contained albumen and casts. She has had one and, I think, two children since without convulsions, but the morbid elements have been found from time to time ever since. The patient was, a few months ago, when I saw her, quite as healthy looking as most women of her age, and had no definite symptoms to complain of.

An attack of scarlatina so slight as hardly to be diagnosticable otherwise, may leave behind, for some time after the patient is apparently well and after albumen has disappeared, a few casts.

The atrophy of old age has already been spoken of.

The difficulties of diagnosis may be considerably increased by the presence of other symptoms, and the resemblance between a case presenting the signs under consideration, with, for instance, severe headache, and a case of genuine Bright's is very close. No general rule can be laid down. A diagnosis depends upon the study of the individual case, and it may require the lapse of time to make it sure.

III. Renal Inadequacy.

The phrase renal inadequacy, used upon our programme, may be made to bear several shades of meaning, one of which, attached to it especially by one whom we hoped would be our guest, I shall for a moment omit from consideration and, for the earlier purposes of this

discussion, shall consider only a supposed inability of the kidney, arising from incipient organic disease, to perform its function of excreting water, urea, or the total solids. I must make this limitation, because the data, which are none too abundant as regards even these substances, are very deficient for any other of the constituents.

What is to be learned from the diminution of any of these constituents that is of value in the diagnosis or prognosis of renal disease? It is, of course, acknowledged, and it is unnecessary to dwell further upon this fact, that an extreme oliguria or anuria is a symptom calling for an immediate investigation, probably, but not necessarily, connected with renal disease, and which may carry with it an extremely serious prognosis. A continued diminution of all the constituents may have less weight in the same direction, but its existence is not to be assumed on the basis of a single hasty examination.

A point of much greater interest, as being less well determined, is whether we can diagnosticate the early stage of chronic nephritis by a moderate diminution in the amount of any of the urinary constituents. The quantity of water is well known to be subject to such individual differences and such fluctuations under various influences that no slight and temporary changes can prove anything, and when the urine in any form of nephritis is permanently diminished to a point decidedly below the physiological limit the diagnosis is usually to be made on other grounds than the quantity of the urine or of any of its constituents alone. Such a condition is likely to occur long after the diagnosis has been clearly made out, but a knowledge of it may be of service in anticipating the onset of other symptoms.

There is, however, a statement made which I believe to be misleading, if not baseless. Polyuria is now a well-recognized symptom of many lesions of the genito-urinary tract, and, barring certain nervous conditions which may produce it by changes in the blood-pressure, and surgical lesions, which act either in a similar way or by the direct effect of back pressure on the secreting structure, and not infrequently lead to a form of nephritis, it is strongly significant of chronic interstitial nephritis. It is one of the most important symptoms, should always lead to further investigation, and the reality of its presence should always be determined by measurement.

This urine is of low specific gravity, and is often said to be deficient in urea. This statement may mean that the percentage is below the

average, which, of course, may be understood of itself, and has no value whatever for diagnosis, so far as single specimens go, or it may mean that the total daily amount of urea is below a certain quantity fixed upon as a standard. The latter ought to be the invariable meaning, but it is not so, and unfortunately it is impossible to tell, in a great many analyses, which of these is intended by the chemist. When, however, the statement is made in this correct sense, it is not to be accepted without reserve until we know what is to be the standard.

The statement of many text-books is that from thirty to forty grammes is a normal, daily excretion of urea for a healthy man on an ordinary diet. This, however, is subject to many corrections, of which the most important (in health) is food. Some French writers assign a lower figure, and intimate that there may be differences of race or habits between the Germans and English on the one hand, and the French on the other. It is much more probably a difference in habits of diet than of race, for there are series of French observations (Roux, *Archives de Physiologie*, vol. vi. p. 593) in which the daily urea is never below 30 for days together, and not infrequently rises above 40.

Without attempting to gather a large number of determinations for the purpose of establishing an average, I have found enough in books upon which I can easily put my hand, to show that a much smaller figure than this is not below the physiological limit: Lehmann (*Physiological Chemistry*), vegetable diet, 22.5; non-nitrogenous, 15.4.[1] Eustratiades (quoted by Rabuteau), averages of successive weeks, 22, 20, 24; under coffee, 19, 17. Rabuteau (*Elements de Therapeutique*, p. 96), 20, 22, 18, 19. Böcker (quoted by Marvaud, *Aliments d'épargne*), 22.27; under coffee, 12.58. Mason (*Boston Med. and Surg. Journal*, 1882, vol. cvii. p. 222), 20, 22.44. Brouardel (*Arch. de Phys.*, vol. viii. p. 388), 18 to 20, or 22. Debove and Flamant (*Gaz. Heb. de Paris*, June 18, 1886) give two tables of the excretion of urea, the first of a patient weighing 58 or 59 kilos, who had a daily quantity of urine varying from 440 to 1240 per diem, with urea from 13.38 to 24.42, and total solids from 24.64 to 39.6. With the second, weighing about 40 kilos, the quantity was from 510 to 1010, and the urea from 13 to 22.58, and total solids from 31.62 to 53.76.

In *The American Journal of the Medical Sciences* for Oct. 1867, Noyes presents several elaborate and interesting tables concerning

[1] These and the subsequent figures refer to grammes in twenty-four hours.

the elimination of urinary constituents by four persons, two males and two females, whom he apparently considered to be in health, or, at any rate, fair subjects for a physiological determination. That he was right in the latter opinion is evident from the fact that none of the numerous figures was sufficiently different from the others to be thrown out, but there are some points about one of them which will make it especially interesting for our purpose. The averages for successive weeks for these four persons were: mixed diet, 15.56 grammes; animal diet, 41.98; vegetable diet, 10.55; exercise (not excessive), 10.32. On two other weeks Noyes determined on himself averages of 15.55 without, and 16.80 with coffee. A seventh week to determine the influence of sleep gave 6.50 for the night, and 8.61 for the day, a total of 15.17. Total solids, corresponding to these weeks, calculated from the specific gravity, are, omitting fractions, 43, 64, 37, 29, 40, 41, 47. It is universally stated that a low diet may reduce these figures much further, so that we may find a patient excreting less than ten grammes of urea per day, without special reference to the condition of the kidney.

It is well known since the recent advances in abdominal surgery that the secreting substance of the kidney may be reduced at least one-half without prejudice to the elimination, and it is, therefore, to be expected that chronic nephritis should have advanced to a considerable degree of atrophy before it begins to make itself manifest by diminution in the secretion. So far as water is concerned, we know that the destruction may be extreme, while the kidneys are still carrying on a discharge of two or threefold the normal amount. For the solid elements it is probable that the capacity may not go quite so far, but the following figures will show that long after nephritis can be clearly diagnosticated the discharge of urea and of total solids may be well above the physiological minimum; indeed, in some cases they may run side by side with the corresponding figures for healthy persons on the same diet. The physiological variations in the urea discharge would indicate very plainly the probability of this. Organs which can, on occasion, double their capacity of discharge as do the kidneys on a full meat diet (take, for instance, cases of diabetes as well as Noyes's figures just given) without harm, certainly ought to be able to lose a considerable percentage of secreting surface before they

become unable to take care of the much smaller but still normal quantity which is formed on a much less nitrogenous diet.

The cases given here are, of course, selected. It is not meant that under all circumstances, in nephritis, the kidneys carry off all the solids formed, but that they can continue to do so long after a diagnosis is possible on other grounds, unless in certain cases in which a nephritis makes considerable advances without the detection of albumen or casts or any other decisive symptoms. Such cases are talked about and are conceivable, but are much more likely to be the results of examinations not having been made than of their having given negative results.

Fleischer (*Deutsches Archiv für klin. Med.*, xxix., 1881) reports the following cases:

A woman, aged thirty-five years, with abundant urine containing albumen and casts and having slight œdema, headache, vertigo, and pain in the back and limbs, had the urine carefully examined daily with comparison with a healthy person on the same diet (Control-person). The urine was always more abundant, although that of the Control-person was quite copious, while for twenty-one days the urea discharged was 32.38 as against 32.33 of the healthy person. The respective quantities of phosphoric acid were 1.64 and 2.51 per cent.

A woman, aged twenty-one years, with a diagnosis both before and after death, of extreme granular atrophy of the kidneys, had for two days 26.2 and 25.2 per cent. of urea, which was less than that of the Control-person but not below the physiological limit. Then there was a period of diminished excretion corresponding to an uræmic attack and, subsequently, on three successive days, it amounted to 43.1, 40.8, 29.7 per diem. This was about three weeks before death, and shows that a very badly damaged kidney can, for a time at least, get rid of a superfluity of urea, and also that in the production of uræmic attacks by retention of the products of secretion something more than the structure of the kidney must have been at fault. The phosphates went through a similar fluctuation.

Another "Schrumpfniere," without a Control-person, passed 30.07 per cent. of urea for seven days. Another, an average of 24.7 per diem.

In another case which seems peculiarly fitted to show the value of different means of diagnosis at an early stage, a man of thirty-one who had albumen and casts but no hypertrophied heart, gave an average for seven days of 40.9 per diem. After a year there was still albumen and his aspect was less blooming, but no symptoms of hypertrophy of the left ventricle could be made out.

A weakly child, eight years old, with great œdema and ascites, the urine containing much albumen, casts, and fatty epithelium, whose post-mortem

diagnosis four months later included pleurisy, peritonitis, fatty and amyloid degeneration of various organs, as well as of the kidneys, passed a daily average of 8.88 per diem of urea against 9.91 per diem of the Control-person.

In these as well as in other cases examined by Fleischer, the diminution of uric acid and phosphoric acid was more constant than that of the urea or the chlorides.

In the interesting tables by Noyes, already cited, the second of the four persons under observation, aged fifty-eight years, constantly passed a urine of low specific gravity and in quantity considerably larger than that of the other persons upon the same diet, being most of the time near or above 2000 c. c. His total daily urea did not fall below that of the others, nor did the proportion per kilo of body-weight constantly do so, and when the amount of meat was much increased, his excretion of urea increased as much as that of the others and became as much above the physiological average as (with the others) it had been below on a vegetable diet.

No examination of the urine for albumen and casts is noted in any of these cases, but so constant an increase of quantity and diminution of specific gravity in a person not stated to have any peculiarity in the matter of beverages, would certainly, at present, be considered of itself alone, very strong presumptive evidence of interstitial nephritis, and when it is learned that this person had hemiplegia two months afterward, it seems highly probable that the author was making some observations of a very valuable kind without realizing that they were pathological. At that time polyuria was not so well recognized a symptom as it is now.

Oppenheim (*Inaug. Thesis*, Bonn, 1881) in a case of acute nephritis found the urea constantly increased—twenty-nine to forty-eight grammes.

In a case of my own, a man of thirty-five, who had been known to have interstitial nephritis for at least two years, I have found this summer, calculating that the two specimens he has each time sent me were fair representatives of the day and night respectively, and that the larger portion is passed during the day, in June, 25.7 per diem of urea and 43 per diem of total solids (the solids by calculation from the specific gravity); in July, 34.4 of urea and 54 of solids; and in August 30 of urea and 53 of solids. As the urine continues to be in about the same quantity I presume that these figures

represent nearly an average of what has been passed for some months. Some days after a hemiplegic attack, the 24 hours urea was 22.5.

We may add to these cases some others in which the urea was not determined but in which the total solids may be calculated from the quantity and the specific gravity.

Phillips's (*Lancet*, Dec. 12, 1885) patient, æt. sixty-seven years, cirrhotic kidneys, 67 grammes solids.

Charpentier's (*Thèse de Paris*, 1880) patient, æt. forty-seven years, headache, hypertrophy of heart, tuberculosis, diagnosis of interstitial nephritis. Total daily solids, 69.

Female, æt. fifty-five years, arterial nephritis. Solids 48.

Female, æt. fifty-five years, interstitial nephritis without albumen. Solids 28.

A male, æt. fifty years, with lead poisoning, emaciated, anæmic, with osteitis of ribs and beginning pulmonary tuberculosis. There was a diagnosis of some renal lesion, the urine was in quantity 2500 and of specific gravity 1.010, which gives the solids as 57 grammes.

In a thoroughly typical case of my own there were found at various times, the last not long before death, 87, 77, 51, 18, 100 of solids.

These cases are certainly enough to show that the excretion of urea and of the total urinary solids does not necessarily fall below that of a person in health on a diet of moderate nitrogenous richness. These, however, are all cases readily diagnosticated, and some of them of long standing and far advanced, so that it may safely be concluded that in cases which are less so, in which the diagnosis is still doubtful, the determination of urea is still less likely to give us assistance.

A suggestion, offered by some of Fleischer's cases, is worth following out: whether some of the other constituents, like uric acid, the phosphates, or sulphates, would give more valuable results. I have made no special examination in this direction, but certainly a deposit of uric acid in urine of low specific gravity is no unusual condition in Bright's disease.

This argument might be much strengthened, if it needed it, by reference to other conditions in which urea is found diminished. In disease of the liver it may fall below the normal, and in some acute affections of this organ it is said to have been absent. In these latter, however, it is highly probable that the kidneys also were profoundly, though rapidly, affected, as in phosphorus poisoning.

Impaired nutrition in general, with anæmia in its various forms, is

the most frequent cause of diminished urea. It is not passed, simply because it is not made, and its deficiency in a given case, in the absence of other symptoms which may fix the diagnosis more exactly, justifies us only in supposing that it is due to some one of many diseases in which nitrogenous metamorphosis is interfered with, of which Bright's disease is only one. In a large proportion of cases a considerable diminution of urea is more significant of disorder of appetite, digestion, and tissue metabolism, than of any failure of the kidneys. There may come a time, and, as we occasionally see, it may come unheralded, when a new set of symptoms indicate that even a small amount of urinary solids can no longer be taken care of by the atrophied, or degenerated, or paralyzed kidneys.

The term renal inadequacy has been recently employed in a somewhat special sense, not exactly corresponding to that in which I have been considering it, by a gentleman who was expected to give us his views in his own words. Sir Andrew Clarke says: "By renal inadequacy I mean that state of the kidney in which it is unable, without material diminution of quantity, to produce a urine containing the average amount of solids, and of a specific gravity greater than 1.014." The urine is described as pale, almost invariably free from albumen, and depositing no casts. The (conjectural) state of the kidneys is one of slight withering and induration. The physiognomy is like that of pernicious anæmia, on the one hand, and, on the other, like that of chronic Bright's, and yet seems distinct from both.

What interpretation are we to put upon this description? If there really are withering and induration of the kidneys, what can they be due to but interstitial nephritis? The account of some of the symptoms would not be inconsistent with the hypothesis of a slowly advancing cirrhotic kidney. We know that this progress may take place without albuminuria for a time, and it is said for a long time; but too much has been made of some exceptional cases. If one were to lay great stress on these exceptions, and view them in the light of the discoveries of the general distribution of albuminuria detailed in the earlier part of this paper, he might be led to the conclusion that but few people fail to present the symptoms of Bright's disease except those who really have it.

With a not inconsiderable experience with chronic nephritis in various forms, I must say that I have not met with many, and I should not be much afraid to say any, cases in which albumen and casts

could never be found, and although a few such cases are detailed as exceptionally interesting, they are not at all common even in literature.

I think it has been very clearly shown that the kidneys are able to carry off the full amount of solids formed long after more than slight shrivelling and induration have taken place; so that if we admit this to be a special form of disease, and if there is any special obstacle to the passage of the urinary solids, it must be of a functional, and not of an organic character, the existence of which is a pure hypothesis, so far as any chronic condition is concerned; or else the lesion is one already advanced, and one which has been making its progress with very few symptoms.

Another very strong point against the assumption that a diminished discharge of the urinary solids in such cases means any inability of the kidney, either functional or organic, is, that it is specially stated that if the supplies of food and wine are cut down, the amount of urea increases. This is easily to be accounted for if we suppose the diminished urea to be due to a form of dyspepsia, or, in other words, if a large amount of food passes off by the intestines without ever being received into the blood, where it can further undergo the metamorphosis into urea. A proper regulation under these circumstances would, undoubtedly, lead to improved nutrition and increased secretion. The specification of wine is somewhat significant in this point of view, since alcohol furnishes but little to the solids of the urine, but may have a very decided effect in impairing the digestion, and is well known, when taken in considerable quantities, to reduce the amount of urea with a possible, but not proportionate, increase of uric acid.

If the kidneys are already inadequate to carry off a small amount of urea, how can a change of diet in the direction of restriction increase it? If more nitrogenous material gets into the blood because the digestion is improved, as has just been supposed, and the kidneys are already inadequate to carry off the smaller amount of urea the only result can be an accumulation and not increased discharge. If, however, the previous digestion has been complete it is difficult to see how a smaller amount of excrementitious solids in the blood can make the amount which passes off any greater. If the kidneys *are* carrying off all they are capable of, then neither increasing nor diminishing the urea of the blood can make them do more. If they *are not* carrying off all they can, then a diminished amount proves no inadequacy on their part to their task, and the deficiency is to be sought in the limited formation.

TABLE I—ALBUMINURIA AMONG PERSONS SUPPOSEDLY HEALTHY.

Author.	Reference.	Class of cases.	Number examined.	Albumen found	Per cent.	Albumen in larger amounts, or by ordinary tests	12–25 c.gr. per litre.	Less than 12, trace.	Remarks
Clarke,	Brit. Med. Journ., Aug. 16, 1887,	Persons expecting examination,	20	3	15	...	...	...	
Leube,	Virchow's Arch., vol. 72,	Healthy soldiers,	119	19	16	Not more than 0.1 per cent.	...	...	After exercise. Before exercise, morning urine 4 per cent. albuminous. Cylinders and blood corpuscles not found.
Fürbringer,	Ztschr. für klin. Med , 1879–80,	Children,	61	7	11.5				
Munn,	Medical Investigations in Life Insurance.	Applicants for life insurance,	630	59	10.6	...	...	...	Tested by boiling.
Kleugden,	Arch. für Psych., 1880–81,	Attendants in asylum,	32	14	43.5	8	...	6	
Grainger Stewart,	Brit. Med. Journ., June 11, 1877,	Soldiers,	205	...	37.6				
" "	" " " " " "	Civilians,	74	...	10.8				
" "	" " " " " "	Children,	40	...	17.5				
" "	" " " " " "	Workhouse inmates aged about 60,	40	...	67.5				
Van Noorden,	Deutsch. Arch. klin. Med., 1885–86,	Soldiers,	53	...	8–10.5	...	...	...	Mucous threads.
Capitan,	Inaug. Thèse, Paris, 1883,	Soldiers,	98	44	45				
"	" " " "	Children,	97	86	89				
De la Celle,	" " " "	Infantry,	120	92	77	...	22	...	
" "	" " " "	Cuirassiers exercising,	88	70	79.5	1	17	...	
" "	" " " "	Soldiers returning from review,	111	100	90	4	...	...	
" "	" " " "	Cuirassiers just vaccinated,	31	31	100	...	10	...	De la Celle In 701 persons in health, 17 give more than 30 cgr. per litre; 152, 12 to 25 cgr. per litre.
" "	" " " "	Students,	50	46	92	...	29	...	
" "	" " " "	Children, 6 to 15,	61	49	80	...	9	...	
" "	" " " "	Children, 6 to 15,	81	62	76.5	...	5	...	
" "	" " " "	Infantry (cold bath)	53	53	100	10	17	...	
" "	" " " "	Soldiers after food,	94	74	79	...	9	...	
Leroux,	Revue de Méd., Paris, 1883,	Children,	330	19	5				
Roberts,	Urinary and Renal Diseases,	Candidates for life insurance,	31	29	...	0	...	...	No albumen by nitric acid or heat. Acid brine (11), picric acid (14), tungstate (28), mercuric iodide (29). Author suspects mucin.

NOTE.—The writer hoped to be able to include the extensive statistics of Dr. Shepherd, of Hartford, but they are not yet published.

TABLE II.—ALBUMINURIA OF ADOLESCENTS, CYCLIC AND ACCIDENTAL ALBUMINURIA.

Author.	Reference.	Class of cases.	Number examined.	Casts found.	Remarks.
Moxon,	Guy's Hospital Reports, xxiii.	Young man,	...	Very few,	Albumen of adolescence; later renal calculus.
Dukes,	Brit. Med Journ., Nov. 12, 1881,	Young men,	..	.....	Some transient; some recover after years, large proportion first [stage Bright's disease.
Johnson,	Brit. Med. Journ., Dec. 6, 1873,	Young men,	...	None,	After bathing.
Vogel,	Virchow's Handbuch, vi.	.....	...	None,	Cases observed by the year. No symptoms. May be slight organic changes.
Pavy,	Lancet, Oct. 17, 1885,	Men mostly young,	...	None,	Cyclic and intermittent Oxalate of lime. No bad health.
Pavy,	Brit. Med. Journ., i. 271, 1876,	Weston (the pedestrian),	...	Abundance, hyaline and granul.	During walk. In 1888 in active business.
Ultzmann,	Wiener med. Presse, Jan. 23, 1870,	Physicians,	8	None,	Urine clear, dark, acid, 1025 to 1035; nothing else abnormal.
Edlefsen,	Mitth. Ver. Schl. Holst. Aertz, 1866 to 1881,	.....	3	Not mentioned; a few times.	Albumen after exertion. Supposes no renal disease (excludes doubtful opalescence).
Fürbringer,	Zeitschr. klin. Med., 1879–80	Officers, young physc'ns, blooming young people,	...	Small cylinders,	Officers after exertion, urine scanty, saturated, containing uric acid gravel.
Marcacci,	L'Imparziale, May 12, 1878,	Young physician (self),	...	None,	Albumen absent from night urine; present in day; increased by exercise [by rest.
Rooke,	Brit. Med. Journ., Oct. 19, 1878,	Young woman, 14 to 16,	...	.....	Intermittent; none in morning; always at night; disappeared
Kinnicutt,	Arch. of Med., 1882,	Young persons,	...	None,	Transient; some associated with high specific gravity; uric acid, oxalate of lime; arterial tension low.
Simmons,	Sacramento Med. Journ., April, 1887,	Young physician,	...	None,	Cyclic; none in early morning; concentrated, four years' standing; now absent; oxalate of lime; uric acid.
Canfield,	Phila. Med. News, 1887	Man, aged 32,	...	None,	Cyclic; none in early morning; maximum at 8 A. M., and in evening; well.
Klemperer,	Deutsch. Arch. f. klin. Med., xii. (as quoted by Canfield),	Student,	...	.....	Cyclic; maxima very nearly as above [also.
Stokes,	Lancet, March 10, 1888,	.....	...	None,	Four years; lithiasis from beer: when lithiasis abates albumen
Stokes,	Lancet, March 10, 1888,	Athlete,	...	.....	Albumen twenty years; now prostatic and cystic trouble.
Ralfe,	Lancet, ii., 1886,	Man,	...	None,	Albumen six years; urine pale, abundant, density high, much pigment
Fergusson,	Brit. Med. Journ, ii., 1878,	Youth of 17,	...	Not mentioned,	Albumen five years; puffiness of eyelids.
Rendall,	Thèse de Paris, 1883,	.....	...	Very rarely,	Albuminuria from digestion (hematogenous).
Engel,	N. Y. Med. Rec., Oct. 1882,	Several young men,	...	None in those in which mention'd	Occasional albuminuria after unusually heavy meal.
Engel,	N. Y. Med. Rec., Oct 1882,	Two young men,	...	Present,	Later symptoms of renal disease more marked.
Merklen,	Med. Press and Circular, p 7, 1885.	Young man, 16 (symptoms acute),	...	None,	Rather weakly; albumen from noon to midnight; more in night.
Fischl,	Deutsch Arch. klin. Med, xxix. 217,	Patients mostly with gastric or hepatic colic, jaundice,	10	None,	Albumen temporary.
Fischl,	Deutsch. Arch. klin. Med., xxix. 217,	Metrorrhagia, collapse,	4	Few,	Albumen temporary.
Bull,	Nordiskt Archiv,	Physician, aged 27,	...	.....	Continued at least two years.
Kinnier,	N Y. Med. Record, June 19, 1886,	Young men,	3	None,	Albumen during middle of day.
Coupland,	Lancet, July 10, 1886,	Girl of 16,	...	None,	No albumen in morning; later, irregularly present; no crystals of uric acid or oxalate.

TABLE III.—ALBUMINURIA AMONG THE SICK.

Author.	Reference.	Class of cases.	Number examined.	Albumen found.	Per cent.
Saundby,	Brit. Med. Journ., May 10, 1879,	Out-patients,	145	105	...
Stewart,	Brit. Med. Journ., Oct. 15, 1887,	Private patients,	150	36	...
Stewart,	" " " "	Indoor infirmary,	150	74	...
Stewart,	" " " "	Outdoor infirmary,	100	19	..
Stewart,	" " " "	Sick children,	50	7	...
Stewart,	" " " "	Fever hospital,	50	33	...
Stewart,	" " " "	Maternity,	25	18	...
Kleugden,	Arch. f Psych., 1880-81,	Male epileptics,	33	13	...
Kleugden,	" " "	Insane,	60	11	...
Capitan,	Inaug. Thèse,	Various diseases, mostly acute,	?	Large number	...
Van Noorden,	Deutsch. Arch. f. klin. Med.	Surgical and ophthalmic cases,	112	...	31.3

TABLE IV.—OPINIONS.

Gull,	Guy's Hospital Reports, xxiii. (Reported by Moxon)	Albuminuria as common as spermatorrhœa.
Johnson,	Brit. Med. Journ., Dec. 13, 1879.	Latent albuminuria means serious trouble.
Teissier,	Lyon Méd., 1887.	Subjects of cyclic albuminuria; probably all destined to become gouty.
De Haviland Hall,	Lancet, Feb. 18, 1888.	Albuminurics should not be assured.
Pye-Smith,	Guy's Hospital Reports, vol. xix.	No person who has albuminous urine (by usual tests) can be considered eligible for life insurance.

VALUE OF ALBUMIN AND CASTS, AND OF RENAL INADEQUACY IN THE DIAGNOSIS AND PROGNOSIS OF KIDNEY DISEASE.

By EDWARD G. JANEWAY, M.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE, AND OF CLINICAL MEDICINE IN BELLEVUE HOSPITAL MEDICAL COLLEGE, NEW YORK.

THE subject presented for discussion is a broad one, and I suppose that no one will dispute the statement, that a due consideration of the amount of albumin, an examination for casts, and an estimation of the performance of kidney function, are essential to the diagnosis and prognosis of kidney disease.

I. ALBUMIN.

If we consider first the diagnostic importance of albumin, we are forced to state that from various circumstances it is apt to be overrated, and, taken by itself, to be, by its presence or its absence, extremely misleading. Particularly is this true of examinations of single samples of urine, or even of one, or of several days' urine, provided such days are not close together; or, on the other hand, not sufficiently separate.

Let us consider, for a few minutes, some of these possibilities. The most interesting is that termed cyclical or intermittent albuminuria. By the method of the albuminuria in these cases, generally present in the daytime and absent at night, it is apt to be overlooked by those who examine only the night and morning urine, and detected by those who examine urine passed in their presence, as at the time of an office visit, or in examination for insurance, etc., or by those making the study of the urine of a large number of individuals. Mistakes are particularly liable to occur between this form and those cases of chronic diffuse nephritis which are attended by intermittent appear-

ance of albumin. An occasional occurrence of a hyaline cast cannot be considered as proof of chronic renal disease as against cyclic albuminuria. Such casts the writer has seen occur in two cases at considerable intervals, the urine not having shown them at a number of previous nor subsequent examinations.

So, too, whilst ordinarily the percentage of albumin is small, yet, in some instances, in one of the cases under the writer's observation it reached twenty-five per cent. by bulk, moist. Neither do the age limits which have been set by some always hold, for the writer has witnessed an instance in a girl of five. A considerable study of the times of its occurrence, and of the urine in which it occurs, is necessary before we can decide in which category we shall place the person presenting albumin as mentioned.

Again, the observations which have been made of the urine of debilitated children, showing that albumin is no uncommon factor in such cases, teach us that the presence of albumin in the diseases of such children does not of necessity depend upon such process, but may have been a preëxisting condition.

Allied to this are those cases of albumin due to disturbed digestion, to nervous influence, to muscular exertion, etc. It seems to the writer that some of those have been illustrations of the cyclic type rather than of the other.

The existence of inflammation of the genito-urinary passages leading as it does to albuminuria, usually of a degree proportional to the amount of blood or pus, is liable to add the next greatest difficulty in relying upon albumin as a diagnostic sign. Three questions present themselves: 1. Is the albumin simply due to the inflammation? Or, 2. Can there be a coexistence of kidney disease? And, 3. If the latter, is it temporary or persistent? In such cases the macroscopic appearances of the urine would be likely to set us right, by presenting a more or less considerable pus deposit; but an absolute decision can not be arrived at without both the use of the microscope and the study of the condition of the urine.

In this connection we would allude to a frequently overlooked source for traces of albumin, viz., that urine containing spermatozoæ may also show a small trace of albumin. Moreover, it is possible to have such albuminuria considered to indicate Bright's disease, particularly if, in conjunction with the ordinary cause, some condition should give

rise to a considerable increase in the amount of albumin, as inflammation of the passages, digestive disturbance, etc. Beale, so far as is known to the writer, was the first to consider this as a possible cause for error.

It is particularly in those conditions of the heart, either primary or secondary, which permit of congestion of the kidney, that mistakes both as regards diagnosis and prognosis occur owing to the existence of albuminuria; and if to this casts are added, and the renal excretion is insufficient, the albumin, etc., is taken to indicate chronic and irreparable renal change. In such cases, as albuminuria coexists with heart disease and weakened circulation, it is inexpedient either to make a diagnosis or prognosis until the heart's action has been strengthened and regulated, so far as this is possible. The writer can, from experience, state that albuminuria, casts, and renal inadequacy have, by presumably competent observers, been taken to indicate chronic and incurable Bright's disease, when regulation of the heart and a supply of fluids have been followed by complete disappearance of all evidences of renal impairment.

The presence of albuminuria in the course of fevers, inflammations, etc., needs no marked mention, but in intermittent hæmatinuria we have a condition which is likely to mislead an expert examiner should a single sample of urine, taken during the paroxysm, or only such urine at different times be submitted to him for examination. More particularly is this true of our more northern climates, where such conditions are comparatively rare. The albumin is not alone present, but casts, blood elements, or blood accompany it.

Again, in the course of diabetes albumin may make its presence in the urine, leading to diagnostic and prognostic errors. The cause of the appearance of such albumin should be carefully sifted, liable as it is to be due, under these circumstances, to irritation of the passages; it may be also due to conditions which give rise to it independent of the diabetes, or, perhaps, be traceable to the same nervous cause which has originated the diabetes. Moreover, in those cases in which it is produced as a result of irritation of the glomeruli, does it necessarily imply that the patient will die of nephritis?

The presence of albumin in small quantity in persons poisoned by lead does not prove that the lead has induced chronic nephritis, possi-

ble as it is that such may be the case. Here, also, a few hyaline casts associated would not demonstrate Bright's disease.

The absence of albumin, or the small quantity present in the urine, may be misleading in diagnosis.

Albumin may be absent or be present in such small quantity not only in the middle portion of the course of chronic diffuse nephritis, but also at the terminal period of the disease. Whilst the ordinary history is that a small quantity of albumin is present, yet its entire absence, with a comparatively high specific gravity, has come under the writer's observation at the end of the disease by coma. So, also, he has witnessed a death from the gouty type, in which he was assured that at no time whilst under observation had there been albumin, or had the quantity or specific gravity borne evidence to the presence of the disease.

So, again, at times albumin is absent from the urine in instances of waxy degeneration of the kidney, and he who would rely upon this as a sure diagnostic sign becomes deceived. There are, moreover, illustrations recorded of acute nephritis without albuminuria. Whether the explanation offered, that the diseased portions are incapable of function, be correct, it is the most plausible. Premising these facts, there are certain conclusions we may draw with some approach to truth.

First. A large quantity of albumin persistent in the urine on several examinations, and large as regards the total day's excretion, and not as regards single emissions, points toward either acute nephritis, the large kidney, white or red, or to waxy degeneration. The appearance of the urine showing evidence of blood would exclude the waxy type. So, also, the color of the albumin precipitated on boiling the unfiltered specimen. Other considerations, and the microscopic examination, would be necessary to decide absolutely.

Second. A small quantity of albumin persistently present in a urine of a comparatively low specific gravity indicates either the convalescent stage of an acute nephritis, or the granular kidney.

II. Casts.

Casts, whilst they are amongst the surest signs of disease of the kidney, may be misleading either by their presence or absence. The question is raised as to the condition of the kidney by certain symptoms

—nervous, of general malaise, or by reason of the presence of a neuro-retinitis, and in consequence the urine is submitted to microscopic examination. If, under these circumstances, a few hyaline casts are found, are we to consider the cerebral disease, etc., as associated with, or dependent upon, a confirmed kidney disease? The writer has met with illustrations which proved that such association of a few hyaline casts was a temporary condition, did not recur, and after the lapse of years the patient's state warrants the assertion that the few hyaline casts were not indicative of a confirmed or incipient nephritis.

Again, a few hyaline casts may be found as the result of examination for life insurance, and the same question is raised. The company's examiners are right to reject such a case as doubtful. Yet it is well to make inquiry as regards the circumstances which have attended their production. The following will illustrate: A gentleman of good physique, fond of sparring, applied for life insurance. His urine examined was found to contain casts. Becoming alarmed he applied to me, and the examination of samples of the night and morning urine failed to show casts, except the soft mucous variety, twice after repeated examinations. Questioning him as regards the time of day at which his urine was examined, and the conditions preceding, it was learned that he was in the habit of boxing violently for half an hour, and perspiring markedly take an alcoholic sponge bath. Urine obtained under similar conditions revealed hyaline casts. After the lapse of four years his urine is of normal character, and had no casts on an examination made this spring. Such a case confirms what experimentation has taught, that hyaline casts may appear in the urine after very slight and temporary changes, quickly form, and as quickly disappear.

Again, the absence of casts on single, it may be on repeated examinations, does not assure us that the kidneys are in perfect condition. It is true that in these cases too often not a large enough amount of urine is allowed to precipitate, and pains enough have not been taken to secure preserved settlings. It is especially true of those hospitals in which ducts made of glazed ware are used, whose interior is foul, and causes the urine to decompose rapidly.

Mention should be made of the soft mucous casts, called cylindroids by some, which are at times found in the urine of patients free from

kidney disease, particularly where the urine is of high specific gravity, and charged with uric acid or oxalate of lime.

All these things considered, are there no sure indications afforded by casts?

We can conclude that if urine contains rich epithelial casts there exists an acute nephritis, rarely an acute exacerbation in a chronic case.

Blood casts speak for an acute process, whether an acute nephritis, or hemorrhage in the course of an older process in the chronic nephritis of large type.

Waxy casts, while they denote a change in the kidney, and hence have a more marked significance than the hyaline and the mucous, may occur in the different forms of kidney disease (Bright's). Present in number, and on different occasions, according to the coincidences, they would point toward either the acute, particularly in its latter stages, or the chronic large kidney, or to a combination of waxy with the parenchymatous nephritis.

Granular casts, coarse and fine, will not be present, or only occasionally in the small kidney, and absent from the urine of the waxy kidney, unless inflammatory changes are occurring in it.

The writer has seen more mistakes made as regards granular casts than of any other variety, by supposing their presence, owing to a confusion with bacterial masses in partially decomposing urine, and this is particularly true of urine containing pavement epithelia turned on its side.

The greatest difficulty will be found in the attempt to discriminate the different varieties of renal disease from single examinations of the urine by the microscope.

Fatty casts, or casts having fatty particles in them, or in the cells connected with them, whilst denoting a degeneration as regards the example seen, and inferentially of more, do not of necessity indicate that the kidney is in an advanced state of fatty degeneration. If such casts are abundant and repeated, then such inference is correct.

The minute crystalline aggregations of uric acid, and more rarely of oxalate of lime, are at times taken for granular casts, and, if a few cylindroids coexist, the kidneys are inferred to be diseased.

Again, the number of casts in a single sample of urine cannot be regarded as giving a correct index to the condition of the kidneys.

Either a markedly diminished secretion may prevent their appearance, though many exist in the tubes; or, on the other hand, owing to a recent reëstablishment of partly suppressed secretion, a large number may make their appearance. Hence, better conclusions can be reached in each of these conditions by several subsequent examinations.

III. Renal Inadequacy.

Incompetency of the kidney is perhaps as, if not more important than the preceding events in estimating the extent of lesion and the prognosis. It cannot, however, particularly in the chronic, nor even in the acute forms, be considered by itself. The condition of the heart forms a marked factor.

Moreover, this state may be simulated by other conditions than those of Bright's disease. Hysterical anuria illustrates this, as well as the effects of occlusion of the ureter. In estimating its importance in a given case, due regard should be had:

1. To the weight of the individual.
2. To the diet.
3. To the effect of profuse discharges.
4. To the effect of rapid absorption of dropsical effusions.
5. To the nutritive activity of the individual.

The sudden entrance of almost complete anuria can be borne for a longer time without severe symptoms, than in those cases in which a gradually increasing deficiency has had place. The cases of removal of a single kidney forcibly illustrate this.

The specific gravity in connection with quantity of urine per diem forms a good measure of renal competency. The relation is not absolute between the urea and the water. The kidney may be much more incompetent for one than for the other. In acute cases, in which the glomeruli are much more affected than the tubes, or perhaps solely affected, whilst the urine is remarkably reduced in quantity the specific gravity may be abnormally high without abnormal ingredients, or other than blood, albumin, and hyaline casts.

After the former of these events we may not even be certain that an inflammation existed, providing recovery takes place. The writer witnessed one case after measles—a child of seven years passed a quart of urine only during a week, of specific gravity 1030. The urine

contained no albumin, blood, nor casts during, or subsequently to this time. The same might be said of the suppression of urine in cholera.

In relation to the diagnosis of the different forms of Bright's disease by the inadequacy of the kidney to perform its function properly many useful rules are laid down in the books; these not thoroughly agreeing, however, because of the discrepancy which prevails about the position of some kidneys, and in proportion as regard is had to the whole course of the disease, or to some particular more typical phase. Liabilities to error exist moreover in the mixed types of kidney trouble as in the small waxy—the combination of waxy with parenchymatous nephritis. In the typical condition a small quantity of urine is characteristic of acute nephritis, but the same may be met with at the terminal period of the contracted kidney, or during the course of the large kidney. So again, during convalescence from acute nephritis, the quantity of urine may be large, and the specific gravity low, producing a simulation of the small kidney as regards that feature. Hence an absolute diagnosis of the type of kidney trouble could not be had by a consideration of the sufficiency of the kidney as demonstrated by the amount of urine and the urea it holds.

But much can be done in effecting a prognosis, particularly in the chronic forms. Here as the end approaches, either in the large kidney, because of the amount of the disease or because of an acute exacerbation, or again as in the contracted kidney, because of the failure of the heart, the growing insufficiency shows itself by the lessened quantity of water, the failure of elevation of specific gravity correspondingly, the failure of urea excretion. In the small kidney in such cases, in many instances, better prognostic events may be gained from the heart's capability than from the urine.

THE RELATION OF ALBUMINURIA TO LIFE ASSURANCE.

By JAMES TYSON, M.D.,

PROFESSOR OF GENERAL PATHOLOGY AND MORBID ANATOMY IN THE UNIVERSITY OF PENNSYLVANIA, ONE OF THE PHYSICIANS TO THE PHILADELPHIA HOSPITAL, ETC.

It is not unusual to hear that medical examiners of life insurance companies have passed candidates who are the subjects of well-established and, it would seem, easily recognized kidney disease; but it may occasion surprise to hear it asserted that a certain number of good risks are lost to companies by the indiscriminate rejection of all cases of albuminuria. This, however, I believe to be strictly true, and it is the object of this paper to point out these, and to discuss the question as to whether it is possible to save them. The importance of such discrimination is manifest, since it will be admitted by every one that no system of life insurance is perfect which does not include all who are essentially healthy, as well as exclude those who are not.

The terms normal or physiological albuminuria, functional, intermittent, cyclical, and alimentary albuminuria have become familiar to every reader of medical literature. They all refer to an albuminuria which is associated with an otherwise apparently healthy organism. No one of them conveys an accurate notion of what is meant, but the term I prefer is *functional* albuminuria.

The words "normal albuminuria" literally mean that the albuminuria itself is normal, which is not intended. The term "intermittent" implies an intermission, which does not always occur, while an albuminuria acknowledged to be abnormal may also be intermittent. The term "cyclical" supposes a periodicity in the intermissions which often occurs, but is by no means constant. Alimentary albuminuria represents a single class of cases in which the albuminuria succeeds the ingestion of food.

Functional albuminuria, on the other hand, simply means that there is somewhere a functional derangement, as the result of which, without structural alteration in the kidney, albumin transudes through its vessel walls along with the normal urine constituents. Of the immediate cause of this I believe it may be safely said we are ignorant, and to speculate upon it is foreign to my present purpose; but I am inclined to believe it is either some quality of the albumin which favors its transudation, or some excess of it in the blood, as the result of which it, so to speak, overflows its banks.

I may allude also, in passing, to certain observations of Ralfe, Noel-Paton, and Oliver, as the result of which it is reasonable to suppose that where albuminuria succeeds the ingestion of proteids it is due to an over-stimulation of the red blood-disk destroying office of the liver whereby these bodies are too rapidly destroyed to permit the resulting albumin to be converted into urea, whence again the blood becomes overcharged, and the albumin escapes.[1] For the present we cannot get nearer the truth than this, but this much is now generally admitted, that the person thus affected is essentially healthy. Let me narrate briefly two cases under my observation which illustrate precisely what I mean.

CASE I.—On March 3, 1883, I first examined, at the request of Dr. W. W. Keen, of Philadelphia, the urine of a gentleman then aged thirty. It had been passed in the morning on rising, and was essentially normal, containing neither albumin, sugar, nor casts. It had 34.5 grammes of urea to the litre. The evening urine contained one-fifteenth of its bulk of albumin but no casts, and there were 2.16 grammes of urea to the litre. Five days later a volumetric examination of the twenty-four hours' fluid discovered 29.8 grammes of urea, the average of two analyses. In addition to the fact that there was no albuminuria on rising, it was also noted that while exercise had the effect of increasing the albumin, massage had not. The most striking symptom was pain in the muscles of his legs, accompanied by a "tired" feeling, in seeking for the cause of which Dr. Keen discovered the albuminuria.

In June, 1884, the patient consulted Prof. T. Grainger Stewart, of Edinburgh, who gave the following opinion: "I have to-day completed the study of ——'s urine. He suffers from intermittent albuminuria, occurring chiefly after food and exercise. He has no evidence of Bright's disease, his urine being of good specific gravity, and containing upward of 400 grains of urea

[1] Ralfe: On Some Clinical Relations of Functional Albuminuria, Lancet, 1886, ii. p. 764. Noel-Paton: Relation of Urea-formation to Bile Secretion, Journal of Anatomy and Physiology, October, 1885, January, 1886. Oliver, Thomas: Relation of Urea to Certain Diseased Processes, British Medical Journal, November 27, 1886.

in the twenty-four hours. There is also no trace of sugar. There is no change in the heart or vessels, or in any organ, and I regard his albuminuria of the functional kind which has been named 'alimentary albuminuria.'"

At Dr. Stewart's suggestion the patient took a long sea voyage. Six months after his return he came under my care. On January 18, 1886, he brought with him, at his first visit, eight specimens, all passed on one day. These I examined with the following result: 7.45 A. M., no albumin; 9 A. M., half an hour after breakfast, one-fifteenth bulk; 10 A. M., an hour after breakfast, one-twentieth to one-fifteenth; 1.30 P. M., after rest, one twentieth to one-fifteenth; 6.30 P. M., after exercise, one-fifteenth; 8 P. M., after supper, one-fifteenth; 10 P. M., *a trace*. I find also the following note: "Mr. C. has no symptoms, except that he becomes easily tired, but he allows his mind to rest a good deal on his albuminuria, and is in the habit of testing his own urine, and says there is much less albumin than there was, that he feels better when at rest and on the ocean."

In March, Mr. C. again consulted Dr. Stewart, at Edinburgh. Dr. Stewart reported his condition about the same, "no better, no worse." He recommended a milk diet, suggesting eight ounces of pure milk and an unlimited amount of skimmed milk or buttermilk. On April 4th, under my directions, he began the milk diet, weighing at the time one hundred and twenty-five and three-quarters pounds. He took forty-eight ounces the first day, gradually increasing the amount until, on April 19th, he was taking four quarts a day. This was continued until April 25th, when I ordered a gradual return to a mixed diet. He first lost weight rather rapidly, losing two and a quarter pounds in six days. But as the quantity of milk was increased, he gained, and when he returned to a mixed diet he weighed within a quarter of a pound of what he did at the start. At no time during this period of twenty days was the urine free from albumin, and although the proportion was less, yet it is to be remembered that the quantity of liquid ingested was very large, and that, therefore, the absolute quantity in the twenty-four hours was probably in no degree diminished.

From this time on until my last note, dated May 7, 1888, the albumin diminished gradually but irregularly so that more frequently at different times of the day there would be none whatever, being always absent on rising; it would be oftener absent after meals, while the average quantity at all examinations was less. His life is always one of considerable activity, and in summer he is extremely active all day long on a country place. At the last examination, at 9.30, after breakfast there was one-twentieth bulk; at 1 P.M., after exercise but not violent, one-fifth; 3 P. M., after dinner none; 7 P. M., after driving, a trace; 9.30 P. M., after supper and rest, one-thirtieth. With slight drawbacks his general health steadily improved, and, early in 1887, he married. *Casts were never found.*

As to the determining causes of increased albuminuria my own study of the case added nothing to the conclusion already arrived at by Prof. Stewart, that food and exercise both tended to increase it, except that possibly exercise exerts a somewhat more potent influence than food.

CASE II., illustrating the class I desire to portray, is that of a gentleman now forty-one years old. For fifteen years his mother has been passing uric acid gravel. Two maternal uncles died when about fifty-eight years old, having had gout since they were forty. The only suggestion of gout shown by him is an occasional tenderness or stiffness about the small joints, with slight lateral swellings.

From 1880 to 1883 I repeatedly examined his urine, finding nothing abnormal, except a slight hyper-acidity, until August, 1883, when I found a trace of albumin by the picric acid test. On each occasion there had been vague tenderness over the region of the right kidney, probably muscular. He is disposed to headache when constipated, on account of which takes care to keep his bowels free. On November 29, 1883, he called upon me complaining of discomfort, rather than pain, in the region of the right kidney, already referred to, a sensation he found difficult to describe. A portion of the past twenty-four hours' urine was acid, had a specific gravity of 1018, was a pale yellow, and contained neither albumin nor casts.

On January 13th, he was examined for life insurance. The medical examiner after his own examination brought me a specimen of urine, perfectly clear, specific gravity 1020, in which I discovered a mere trace of albumin by the contact method with picric acid, and by the heat and acid test. On the 18th of the same month, the patient himself brought me a specimen, which also contained a trace of albumin by heat and acid, but which did not respond to nitric acid alone. The quantity on subsidence was decidedly less than that in the urine brought me by the life insurance examiner. On January 20th the urine responded to neither test. *None of these specimens contained casts*, but considerable uric acid appeared some time after the urine was passed.

On January 25th, my patient brought two specimens of urine, one voided in the evening just after tea, the other on rising Saturday morning. Neither contained albumin. On Saturday evening he went to a dinner party, ate freely of rich food, and drank champagne. After dinner he played on the violin for two hours, music requiring much expression and concentration. The urine passed the same night on retiring contained one-tenth bulk of albumin, and that brought to me had a copious sediment of urates. In that of the next morning there was a trace of albumin, but the most careful examination failed to discover casts. Urine passed Sunday evening after a supper of crackers, coffee, and preserves furnished a trace, as also did that of Monday morning. A portion of the twenty-four hours' urine also contained a trace.

Two days later, on rising the urine was absolutely free from albumin; but after a breakfast of tripe, bread, and milk I found one-tenth its bulk. At 2.15 P.M., having attended a board meeting from 12 to 1.30, there was a mere trace. At 5.30 P.M., dinner and office business intervening, there was no albumin; nor was there any at bedtime.

The next day on rising there was absolutely none. That passed at 10 A.M., the first passed after breakfast consisting of a chop, oatmeal, and cream fol-

lowed by pepsin and a railroad journey of forty miles, was without albumin. After one and a half hours in court testifying, no albumin. After dinner consisting of chicken, bread, milk, and potatoes, none. That passed on returning to Philadelphia contained none. After a tea of oysters and milk-toast there was a trace, and at 12.30, after a visit, also a trace, rather more than after tea.

Such notes as these were made from day to day, and may be regarded as fair samples of the average condition. Always, with the exceptions named, the urine was without albumin on rising, but it was impossible to discover any law followed by the albuminuria at other times. At one time after breakfast, although at home writing quietly, there was one-tenth of albumin; at another there was none. On one day there would be a trace at each examination; at another, although more rarely, there would be none. This important fact was, however, ascertained, although the urine passed on rising contained no albumin, that passed at 9.30 A. M., *but before any food had been taken*, contained one-seventh its bulk. Urine passed at 11.05 A. M. the same day, one and a half hours after breakfast, contained a *trace*; that passed at 2.30 P. M. contained a mere trace; at 4.45 P. M., two hours after dinner, none; and at 6 P. M. none; that passed after tea contained none; at 11.45 P. M., after a lunch of roasted oysters, weak coffee, bread, and a teaspoonful of sherry wine a trace. The last urine had a specific gravity of 1030. This day's observations showed that the albuminuria did not, at least, necessarily depend on food; but rather that exercise was responsible for it. At other times, however, after exercise and after food there was no albumin. It was observed that there was almost invariably a tendency to increase after breakfast under any circumstances, almost as invariable as there was absence on rising. Next to the absence of albumin on rising, absence was noted more frequently between 4 and 6 P. M. More commonly there was albumin when the specific gravity was high, yet I note that at least once with specific gravity of 1036 there was none.

In this case the albumin also became gradually less, and ultimately disappeared. Quite an important observation was this, that the use of "beef-tea" or "beef-essence," as prepared at home, invariably increased the albuminuria.

Contrasting these cases I would call attention to the fact that although in the matter of family history and age the second case would be regarded as more unfavorable, yet in it the albumin disappeared altogether, while in the first it is still intermittently present.

These cases I regard as illustrating a class in which, although there is albuminuria, its subjects have not renal disease and are insurable risks. Such conclusion is, however, based upon a study of the cases extending over months in the one instance, and years in the other, a study which is seldom, if ever, possible to the ordinary medical examiner. There can be no doubt, however, that could a set of rules be

laid down for his guidance the well-trained examiner might save to his company a certain number of risks which are now lost by it. Unfortunately from necessity perhaps, at the present day, the majority of medical examiners are neither well educated nor well trained. Supposing, however, that these important desiderata are attained, as it is reasonable to believe they will at no distant day, are there any conditions which if observed will enable the companies to secure these risks which are now rejected? I believe there are and I submit them to your consideration.

First. It is scarcely necessary to say that the applicant must in all other respects present the signs of good health.

Second. The albuminuria should be unaccompanied by tube casts. However perfect may appear the health of an applicant with albuminuria, the presence of casts in his urine must effectually close the gates of life insurance against him. Casts and albumin conjoined can receive only one interpretation—structural change in the kidney either acute or chronic. Here, however, let me say I have known men with good medical education say that casts were passed in a given specimen when there were none; certain granular aggregations having been mistaken for them.

Third. If the quantity of albumin is large, the applicant should be rejected irrespective of the presence of casts. The term large albuminuria admits, of course, of considerable latitude in its application; but I should say that an albuminuria is large in which the albumin habitually exceeds one-fifth the bulk of the specimen examined.

Fourth. A consideration which goes far toward establishing the functional character of an albuminuria, although not essential to this end, is the absence of albumin on rising in the morning. Nor dare it, of course, be said that such an albuminuria precludes the existence of organic disease. It must be taken in connection with the other considerations mentioned.

A *fifth* point of importance is specific gravity, meaning thereby the specific gravity of the twenty-four hours' urine, or what my friend Dr. Charles W. Purdy, of Chicago, calls the "real" specific gravity. The specific gravity of any portion of the twenty-four hours' urine, or the "apparent" specific gravity, is of limited value, varying greatly with the time of day and relation to meals. If we regard the specific gravity of a normal twenty-four hours' urine (say 50 oz.) as 1020,

the following may be laid down: *albuminuria is least significant when the specific gravity is high*, throwing out, of course, the consideration of sugar. The specific gravity is a direct measure of the solids, which in health are strikingly constant for each twenty-four hours, while the same is true in a less degree of the watery part of the secretion. Hence, the specific gravity of the twenty-four hours' urine is tolerably constant. In all forms of renal disease the solids are diminished, and in all except acute nephritis and cyanotic induration the specific gravity is lowered. In these two it is lowered because in them the quantity of urine is also markedly less, and, in consequence, the specific gravity is increased to 1028 and even higher. In acute nephritis the presence of blood may also contribute to such specific gravity. Strictly speaking, acute nephritis may be ignored, because no one ill of it is likely ever to apply for life insurance. In functional albuminuria the specific gravity remains normal. Dr. Purdy even says that his observations in many of these cases show a specific gravity above the normal standard, reaching 1022 to 1030.

Whenever, therefore, in addition to the absence of other symptoms and signs alluded to, the "real" specific gravity is above 1020, another most important fact is in evidence against the presence of organic disease and in favor of the view that the albuminuria is functional. On the other hand, if the real specific gravity of the urine be 1012, 1010, 1008, or even less, as it sometimes is, it would be hazardous to to accept such a case of albuminuria, however good may be the apparent health of the applicant and even in the absence of casts. To judge unfavorably from the specific gravity of any portion of the twenty-four hours' urine would be exceedingly unfair to the candidate.

Sixth. The signs of hypertrophy of the left ventricle and of high vascular tension associated with albuminuria are conclusive symptoms of renal disease and should exclude the candidate. High vascular tension may sometimes be shown to exist by the sphygmograph, even when not appreciable to the touch.

Seventh. A highly important consideration is the age of the candidate. Albuminuria is much less apt to be of the functional kind in persons over forty years of age than in those who are younger, especially between the ages of twenty-one and thirty-five. It is questionable whether any person with seemingly functional albuminuria who

has reached the age of forty should be accepted unless he has been long under the observation of a competent and conscientious observer.

Eighth. The presence of true gout in any shape precludes admission to life insurance, because gout is always sooner or later followed by interstitial nephritis.

Finally. The retinal symptoms so commonly associated with chronic Bright's disease, although usually late in their appearance, do sometimes form the earliest noted sign of this affection and, whether or not conjoined with albuminuria, must effectually exclude the candidate from the advantages of life insurance.

These, in brief, are the conditions which, if observed, appear to me to enable one to establish conclusively the functional character of an albuminuria, the presence of which should not exclude a candidate from life assurance. They might have been further elaborated, but I prefer to leave them to be discussed as presented. It is to be remembered, however, that observations should extend over a considerable period of time; also, that there is reasonable doubt as to whether the requisite knowledge and training are sufficiently general to secure intelligent recognition of the conditions. Take, for example, so simple a matter as examining the tube casts, which may be regarded as almost the easiest sort of microscopical work. Yet, how few medical examiners are competent to make such examinations. Most of them have no microscopes, and many who have them use them so rarely that their opinion whether positive or negative as to the presence of casts cannot be relied upon. On the other hand, there are those who are competent to apply these conditions, and it is reasonable to suppose that this number will gradually increase.

I conclude, therefore, that life assurance associations should recognize a condition of functional albuminuria out of consideration of their own interest, as well as in justice to a class which is coming to be recognized as more numerous as our knowledge increases, and who are blindly excluded from benefits to which they are entitled.

DISCUSSION.

DR. A. JACOBI: To criticise papers of such excellence would be impossible, but I may be permitted to recall a simple anatomical fact which may explain the frequency at least in part of inflammatory disease of the kidneys and the presence of albumin in children. It is true children have diseases in which kidney trouble is very common. The anatomical fact to which I refer is that immediately after birth there is a peculiar discrepancy between the development of the renal artery and the growth of the kidney. The size of the renal artery develops much faster than the renal tissue grows in bulk. Besides, the capillaries of the kidneys of the young do not permit the same current of water to be pressed through them in the same time as the capillaries of the adult. Thus congestive processes are more readily brought about.

DR. PYE SMITH, of London: I quite agree with the author of the paper, as to "functional albuminuria" being, at present, the most convenient term for that condition, because it depends upon several states differing from one another and only agreeing in the practical fact that none of them implies structural disease of the kidney. Physiological albuminuria is a mischievous term because it leads to the idea that we can overlook, as a normal condition, what certainly should always awaken grave anxiety. At present there is no physiological ground for believing that a healthy kidney secretes albumin. Those of us who have seen cases of so-called physiological albuminuria will agree that they must be carefully watched in order to prevent them from going from bad to worse and involving the kidney in more serious disorder. As physician to a London insurance company, I have tried many tests for albumin in the urine, and I believe that the old tests of heat and nitric acid are the most satisfactory of all. If we introduce new tests and new chemical criteria, we imperil the clinical and pathological results of the past fifty years.

DR. F. P. KINNICUTT: Seven or eight years ago I published a series of cases of temporary albuminuria occurring in children and adolescents in apparently good health. In all, the urine contained albumin in a very considerable amount, but casts were never detected. I have been able to keep these patients under observation up to the present date. In a single case, occasionally a temporary albuminuria still manifested itself, the health remaining excellent. In the remaining cases, there has been a cessation of this symptom. The observations seemed of value to me, as personally I have never been able to divest myself of an anxiety in regard to the future of such cases.

DR. F. PEYRE PORCHER: In yellow fever albuminuria exists and is one of the most dangerous symptoms of that disease. I think if yellow fever were treated during the *first ten hours* by large doses of mercury, quinine, and *cold water*, followed by a single saline purge—Blair's plan modified—the mortality might be reduced to as little as in any disease that exists. The albuminuria and the black vomit are the products of the high temperature and the insidious nature of the initial stage.

THE CARDIAC CHANGES IN CHRONIC BRIGHT'S DISEASE.

By ALFRED L. LOOMIS, M.D., LL.D.,
PROFESSOR OF PATHOLOGY AND PRACTICE OF MEDICINE IN THE UNIVERSITY OF THE CITY OF NEW YORK.

THERE is no more important fact to be remembered, or one which can form a safer basis for argument in our consideration of disease, than the fundamental law of self-preservation. Viewed from such a standpoint, many so-called pathological processes become physiological, in that they tend to the preservation of health and prolongation of life. It is only when an exhausted vitality, from overactivity or failing nutrition, passes into degeneration of structure, that these processes tend toward death rather than life. As the centre of the entire system, the heart furnishes the most perfect illustration of this principle. So long as full nutrition is maintained, it meets every obstacle to its activity with added force, and only when its walls are weakened do we find that all of nature's pathological efforts have been put forth only to end in that evidence of exhausted vitality, cardiac insufficiency.

As one considers that it is a failing, and not an hypertrophied heart which is to be feared in Bright's, as in other diseases, one can but be impressed with the fact, when comparing the opinions of the various writers upon the relations of cardiac and kidney disease, that, almost without an exception, they have made little or no reference to cardiac degeneration, except as secondary to hypertrophy, and dependent either upon excessive muscular strain or malnutrition from obstructed coronary arteries. Cardiac hypertrophy, on the other hand, receives marked attention, and is discussed as though it were of the utmost moment. Many most ingenious theories have been advanced to account for a process which is always conservative and compensatory, and which doubtless we would not arrest if we could, save by the removal of the

condition for which it compensates, while it has almost been taken for granted that the degeneration and failure are but the epilogue to a tragedy that is almost ended. Even so late an authority as Fagge makes no mention of cardiac degeneration as one of the important changes in Bright's disease, but specifically states that "the great point to be insisted upon is that hypertrophy of the heart occurs both in parenchymatous nephritis and renal cirrhosis." Strümpel also says, "We now know that cardiac hypertrophy is not confined to contracted kidney, but that it is almost as constant in many other forms of nephritis," but he makes no mention of degeneration as a primary or secondary change. The arterial changes so often met with in chronic Bright's disease, even from the time of Bright himself, have been looked upon as important concomitants of the renal lesions, but within the past decade they have attracted so much attention as to be regarded by some as the most important part of the pathological processes. Some have gone so far as even to speak of Bright's disease without lesions of the kidney. That compensating hypertrophy of the left ventricle is a pathological necessity of such arterial changes seems evident; and if those changes in the glandular structure of the kidneys which are usually included under the general term of chronic Bright's disease are associated with such arterial changes, cardiac hypertrophy will be a necessary part of the history.

My clinical and pathological studies during the past five years, however, have forced upon me the conviction that simple cardiac hypertrophy is not found in so large a proportion of cases of chronic Bright's disease in this country as are reported by English and Continental observers; and that when it is associated with extensive changes in the glandular structure of the kidney it is due to the primary arterial rather than to the renal disease. The most constant cardiac changes which I have met with in chronic Bright's disease are dilatation of the heart cavities and degeneration of the heart walls. Generally when I have found tubular and interstitial changes in the kidneys in addition to obliterating arteritis, cardiac hypertrophy, when present, was associated with degenerative changes in the heart walls and dilated cavities.

In those forms of renal disease in which the principal changes were in the tubes, Malpighian capsule, and connective-tissue stroma, with little obliterating change in the vessels, I have found no true cardiac

hypertrophy, but rather the changes which are characteristic of a Bright's heart.

One who studies Bright's disease clinically in our large hospitals soon realizes that the heaving impulse and the increased arterial tension, which seem to be the essential features of the disease as described by many writers, are the exception rather than the rule, for in most of such cases it is difficult to determine the maximum point of cardiac impulse, it is so diffused and indistinct; and, although the cardiac area of dulness may be increased, the other signs of hypertrophy are wanting, and when, at the autopsy, a large flabby heart is found, one comes to look for other cardic changes in chronic Bright's disease than muscular hypertrophy.

In making so broad a claim as that hypertrophy is not the characteristic cardiac change in chronic Bright's disease, it seems necessary to consider the interdependencies which have been assumed to exist between cardiac, arterial, and kidney disease, and to point out how many of them, rightly interpreted, support our present proposition.

The early supposition of Traube, that the obstruction of the renal circulation alone was sufficient explanation of the increased arterial tension and cardiac change, was but partially accepted, and was generally abandoned when the recognition of a general arterial fibrosis led to a more comprehensive understanding and explanation of the associated processes. Although the description of those pathological changes given by Gull and Sutton was shown to be incorrect, their comprehensive explanation of a general arterial disease coincident with the renal changes as the cause of the cardiac hypertrophy was fully accepted. It was even carried so far by Mahomed as the assumption that the kidney changes were either an etiological sequence of, or simply associated with, the more important systemic condition.

Gull and Sutton, however, recognize the fact that the cardio-vascular changes vary greatly as the kidney lesions affect the vascular or tubular structures, and ascribe the hypertrophy found in parenchymatous nephritis in part to weakening of the circulation through a general œdema of the tissues and malnutrition from anæmia and uræmia, a point which I wish especially to emphasize. While high arterial tension throughout the body thus becomes the recognized cause of the cardiac hypertrophy, the explanation both of its cause and nature was various and contradictory. So long as the renal disease

was regarded as primary, the arterial thickening was considered as inflammatory in character, and due simply to the direct irritation of excrementitious products in the blood upon the delicate capillary walls. When, however, the arterial disease was supposed to be primary and the kidney disease secondary, an irritation must be found elsewhere. An excessive production of uric acid, by its presence in the blood in irritating quantities, as readily explained it as did faulty eliminations; and the recognized etiological relations of gout and the uric acid diathesis to granular kidney thus suggested deficient hepatic functions, either inherited or acquired, as the starting-point in the chain of pathological processes which eventuate in both cardio-vascular disease and coincident granular kidney.

That such is the order of change from uric acid, scarlatinal, alcoholic, and other systemic poisons, as claimed by Mahomed, seems indisputable. Indeed, Riegel (*Zeitsch. für klin. Med.*, vol. vii. p. 210, 1884) affirms that the severity and extent of the tubular change in the kidneys may be more accurately determined by the increase in arterial tension than by the amount of albumin present in the urine.

In 1886 Dr. George Johnston states that the thickening of the arterial walls which was found in muscles, brain, and tissues, generally in connection with kidney disease, was due to the hypertrophy of the muscular coat. In this he was supported by Waller (Virchow's *Archiv*, 1887), who states that the change is a true hypertrophy, but not a hyperplasia. Others (Lotnischewsky, Virchow's *Archiv*, 1880), however, deny the presence of any muscular hypertrophy in the arteries.

Dr. Johnston's own explanation of the muscular hypertrophy—that it is due to an attempt on the part of the system to shut off from the tissues blood which is irritating by reason of being surcharged with excrementitious products—seems, *à priori*, untenable. The possibility of such a change, however, even when resting upon no further authority, becomes of extreme interest in connection with the more recent theory of Cohnheim concerning the cause of high arterial tension.

The two together afford an explanation of the arterio-cardiac changes in all cases when cardiac hypertrophy is secondary to kidney disease, which, to our mind, is more in accord with well-recognized

principles of pathological processes than any which has yet been presented for our acceptance.

Briefly stated, Cohnheim's affirmation is that the circulation in the kidneys, as in all other glands, varies with the demands for their functional activity.

Recent physiology teaches that while increased blood-supply to any gland may stimulate its functional activity, stimulation of its function, whether direct or indirect, to a far greater degree increases its blood-supply. The circulation through the kidneys at any given time will thus vary directly as the proportion between the parenchymatous elements of the kidneys and the amount of excrementitious matter contained in the blood. Any diminution of the amount of glandular tissue in the kidneys at once raises the ratio between the excrementitious matters and the functional power of the kidneys. As a direct result, when the lessening of renal functioning is coexistent with retention of excrementitious material, the systemic forces generally are projected upon the kidneys in order that the total elimination may remain undiminished.

It seems but reasonable to suppose that here, as elsewhere, such a sympathetic control of a local circulation should take place through the nervous system and the muscular elements in the vessels rather than by degenerative changes and fibroid thickening. Such vaso-motor changes would not in any degree interfere in fibroid changes dependent upon direct irritation.

We thus have two widely different conditions tending to increase arterial tension, conditions which may act singly or conjointly, and affording a rational explanation of the varying amounts of cardiac hypertrophy found in apparently similar renal conditions: *First*, a general and primary arterial sclerosis, to which both the cardiac and renal changes are secondary; and, *second*, arterial as well as cardiac hypertrophy, which are as compensatory and conservative as is cardiac hypertrophy in aortic lesions.

Recently Da Costa claims to have established a connection between certain degenerative changes in the ganglionic centres and both the renal lesions and cardiac hypertrophy. Without accepting or attempting to deny his claims as to the pathological changes found in the nervous tissues, we are unable to see that he has established any causal relations between such changes and the cardio-vascular condi-

tions. The evident clinical relations between lithæmia and both the vascular and renal changes would not seem to be affected even by allowing his claims. Nearly all authorities agree that parenchymatous nephritis is far less frequently associated with the cardiac hypertrophy than is cirrhosis, a fact which does not at all accord with the supposition that cardiac hypertrophy is dependent alone upon increased arterial tension secondary to retention of urea in the circulation.

Senator claims to have determined that the hypertrophy of parenchymatous nephritis is eccentric, while that of renal cirrhosis is simple or even concentric, implying a concomitant degeneration of the cardiac tissue in the former case, and an apparent rather than actual hypertrophy in the latter. It is quite well established that in many cases the fibroid changes are not confined to the arteries, but are associated with similar changes in the cardiac walls. Such examples are evidence of primary cardiac degeneration associated with the renal disease, either dependent upon the kidney lesion or some more general cause.

While most of the theories, as well as the clinical evidence thus adduced, seemed to be directed to establishing a cause for the arterial changes and increased tension, many of the facts presented appear to me to support the proposition that dilatation of the heart cavities with failure of the muscular power of the heart walls is, perhaps, the characteristic cardiac change in chronic Bright's disease.

Before presenting the tabulated report upon which many of the conclusions in this paper are based, I desire to offer observations made in a series of cases that I regard as typical of the different varieties of kidney and cardiac changes which are found associated in chronic Bright's disease.

CASE I.—My first observation was made in a stock-broker, forty-four years of age, who applied for life insurance on November 3, 1885, and was rejected upon the ground that his urine contained albumin. The following day he presented himself at my office for examination.

He stated that he had no hereditary tendencies to disease, that he had always been a generous liver, had never been ill, except with a slight attack of gout in the right great toe-joint about a year before; so far as he knew, he was in perfect health.

The only evidence of disease which could be appreciated was an enlarged, forcible, and prolonged apex-beat, its maximum point of intensity being two inches below and an inch to the left of the line of the left nipple. The area

of cardiac dulness extended beyond the left nipple. The first sound of the heart was prolonged, dull, distinct, and reduplicated. The second was unusually distinct. There was markedly increased arterial tension, as indicated by the pulse and sphygmographic tracings; the specific gravity of his urine ranged from 1010 to 1016. The average daily amount passed for four consecutive days was forty-two ounces. It always contained a moderate amount of albumin, but at repeated examinations no casts were found. Ten days after he came under my observation, while walking on the avenue with a friend, he fell in an unconscious state, and in a few moments was dead.

The *autopsy*, sixteen hours after death, revealed a large blood-clot, filling both ventricles of the brain. The heart, after its cavities were opened and emptied of blood, weighed twenty-one ounces. The left ventricle was markedly increased in size; its cavity was not enlarged, but its walls were greatly thickened. The middle portion of its anterior wall was an inch and a quarter in thickness. Its cut surface was of normal color and presented the appearance of healthy muscle; microscopical sections at different points showed simple muscular hypertrophy. The right ventricle was of normal size and appearance. The valves were normal and no change could be detected in the coronary arteries. The kidneys were slightly enlarged, of an abnormally dark color, their capsules non-adherent, and their surfaces smooth. On section, the cortical substance seemed somewhat increased, but the markings were distinct. Microscopical sections, both of cortical and medullary substance, showed extensive obliterating changes in the arteries; no abnormal changes could be found in the renal tubes or in the stroma of the organs, except a moderate cell infiltration around some of the tufts. Extensive obliterating changes were found in the cerebral vessels.

CASE II.—A similar observation was made in a janitor of an apartment-house, who was admitted into Bellevue Hospital, March 5, 1886. He was forty-five years of age, had been a moderate drinker for twenty years, but still retained a fine physique. For the past year he had suffered with frequent and severe headaches; otherwise he had been well. Five days before admission to the hospital, while stooping, raking ashes from a furnace, he felt dizzy, attempted to rise, and fell forward; was unconscious for thirty-six hours. When he became conscious he found it difficult to speak, and was unable to move the right side. When admitted to the hospital there was a complete right-side hemiplegia, and the physical evidence of extensive left cardiac hypertrophy, but no cardiac murmur. The pulse and sphygmograph gave evidence of increased arterial tension. The specific gravity of his urine was 1010. It contained ten per cent. of albumin and a few hyaline casts. There was slight œdema along the line of the tibia. Without any appreciable change taking place in his condition, eight days after his admission he died suddenly.

At the *autopsy*, twenty-four hours after death, the left lateral ventricle of the brain was found filled with a recent blood-clot, which involved the left corpus striatum. The vessels of the base of the brain, the only ones examined,

showed extensive obliterating arteritis. The left ventricle of the heart was greatly enlarged; after the heart cavities were opened and emptied it weighed nineteen and one-quarter ounces. The anterior wall of the left ventricle at its middle portion was one and one-eighth of an inch in thickness, and the papillary muscles were greatly enlarged; the ventricular cavity seemed of normal size. The right ventricle presented a normal appearance; microscopical sections of the walls of the left ventricle at different points showed the muscular fibres increased in size, their striæ distinct, and their nuclei enlarged; there was no connective tissue increase. The kidneys weighed nine and one-half ounces; their surface was smooth, their capsules non-adherent, and on section their markings were distinct. Microscopical sections of both their medullary and cortical substance showed extensive obliterating changes in most of the renal arterioles. The tubules presented a normal appearance. There were in some of the tufts a slight amyloid change and a moderate round-cell infiltration.

These two cases are striking examples of extensive hypertrophy of the left ventricle, associated with obliterating changes in most of the renal arterioles, with but slight changes in the glandular structure of the kidneys. The fact that these arterial changes were similar to those which are often found in the advanced stage of cirrhotic kidney, and were associated with similar vascular changes in other organs, furnishes a reasonable solution for the cardiac hypertrophies which are so often present with the cirrhotic kidney. Such cardiac hypertrophies are not a part of the pathological history of the tubular and structural changes which take place in the granular kidney, but belong to the general fibrosis and vascular changes which *precede their development.*

CASE III.—My third observation was made in the case of a lawyer, thirty-seven years of age, a generous liver, with a strong hereditary tendency to arterial sclerosis, his father and four of his uncles having died before sixty with cirrhotic kidneys, associated with cardiac disease and obliterating arteritis. They all had been under my medical care during life. I saw him in his first severe illness (an attack of general bronchitis) in January, 1886. At this examination, I found, in addition to the evidences of bronchitis, extensive left cardiac hypertrophy unattended by cardiac murmur, but associated with very marked arterial tension. The sphygmographic tracings of high tension were typical. The specific gravity of his urine was 1024; it was scanty, strongly acid, and contained ten per cent. of albumin and a few hyaline casts. He complained of wandering pains in his joints, but they were not swollen or tender. Under small doses of calomel and colchicum, his diet being restricted to Vichy and milk, his bronchitis disappeared in a few days. His urine became normal in quantity, less acid, and no longer contained albumin, and the arterial tension somewhat diminished.

He continued under my occasional observation for several months, and seemed quite well. Frequent examinations of his urine during this period gave negative results. The area of cardiac dulness, however, steadily increased, and the other signs of cardiac hypertrophy became more pronounced. In October, 1887, he came to me complaining of shortness of breath, and said that he had noticed for a few days that his vision was indistinct. He stated that he had been unusually well during the summer, until about ten days before. After a long row, he had sat in an open boat, in a strong wind, fishing, had become chilled, and since had suffered from a constant and severe headache, nausea, and shortness of breath; had noticed that his face was swollen in the morning. His pulse no longer gave evidence of arterial tension, but was feeble, irregular in force and rhythm; his countenance had a languid expression, and he said that food was repulsive to him. I was astonished at the change that had occurred in his heart since my last examination. The impulse was diffused and indistinct. The area of cardiac dulness was greatly increased latterly. The first sound of the heart was indistinct, and closely resembled the second in quality. His urine was scanty, specific gravity 1026, contained twenty-five per cent. of albumin, epithelial and granular casts. His ankles were œdematous, and his face puffy; no sign of œdema of the lungs. An ophthalmoscopic examination revealed slight neuroretinitis. He rapidly grew worse, and six weeks after died, with the usual symptoms of acute Bright's disease.

At the *autopsy*, eighteen hours after death, the heart weighed twenty ounces; both ventricles were markedly dilated. The walls of the left ventricle were three-quarters of an inch in thickness; the right an eighth of an inch. The cut surfaces of both were paler than normal, and less firm. Microscopical sections of different points showed the muscular fibres of normal size, their striæ indistinct, and their nuclei granular. The connective tissue in the walls of the left ventricle was uniformly increased; the walls of the coronary arteries at their origin were somewhat thickened. The kidneys were enlarged, and weighed sixteen ounces. Their surface was smooth, and their capsules non-adherent. On section, their cortical substance was markedly increased, and of light color. Most of the renal arterioles were thickened, and at points obliterated. The epithelium of the convoluted and straight tubes, and that of the Malpighian bodies, was swollen and granular, and in some of the tubes detached from their walls. There was slight connective-tissue increase. The vessels at the base of the brain showed obliterating changes.

This case is an example of extensive obliterating changes in the renal arterioles associated with the characteristic changes of acute parenchymatous nephritis. With these kidney changes there were extensive cardiac dilatation, moderate thickening of the cardiac walls, and degenerative changes in the cardiac tissue. The natural explanation of these changes is that the obliterating arteritis had been gradually but progressively developing for years, the result of a strong

hereditary predisposition to general arterial sclerosis. As a consequence, a compensating hypertrophy was developed. On the occurrence of the parenchymatous changes in the kidneys, the nutritive power of the blood became so diminished that all the organs and tissues of the body, especially the heart, became enfeebled, and cardiac dilatation and feeble heart were the result. How much the functional nerve disturbance caused by the attending toxæmia had to do with the heart failure I am not prepared to state.

CASE IV.—My fourth observation was made in the case of a fireman, fifty-six years of age, of intemperate habits, who was admitted into Bellevue Hospital on April 23, 1887, with extreme dyspnœa, cyanosis, and general anasarca. He stated that he first noticed swelling of his feet and face three years before; that at that time his urine was scanty and high-colored; he had nausea, vomiting, and headache. These symptoms were relieved in a few days, but afterward he was never quite well, always having some swelling of his feet, and trouble with his stomach. One year ago he had an attack of pneumonia. At the time of his admission to the hospital his temperature was 99°; pulse 130, small, weak, irregular, and intermitting; respirations 72. His countenance was anxious, his lips markedly cyanosed. Œdema of the lower extremities extended up to the middle of the thighs. Fluids or solids taken into the stomach were immediately rejected. There were the physical evidences of general bronchitis and œdema at the base of both lungs. The cardiac impulse was diffused, feeble, and the maximum point of apex-beat could not be determined. The area of cardiac dulness was markedly increased. The heart sounds were equal in intensity and duration; there was a blowing systolic murmur at the apex. The specific gravity of his urine was 1018, and it was completely solidified by heat. It contained blood-corpuscles, epithelial and fine, granular casts.

At the *autopsy*, twenty-four hours after death, the heart weighed nineteen and a half ounces; the anterior wall of the left ventricle was five-eighths of an inch in thickness; all its cavities were dilated, especially the right ventricle. There were a few small atheromatous patches at the base of the aortic valves and in the walls of the aorta, which was somewhat dilated. The valves otherwise were normal. The muscular fibres of the ventricles were atrophied and granular. The walls of the coronary arteries were atheromatous and their calibre somewhat diminished. The kidneys were typical specimens of the large white kidney. They weighed eighteen and a quarter ounces, their capsules were slightly adherent, their surfaces smooth, pale, and mottled. The cortex was increased and the markings indistinct. Most of the tubules were denuded of their epithelium; some contained cast matter. The connective tissue between the tubes and around the tufts was markedly increased; the external coat of the arterioles was thickened, but there was no obliterating change in the vessels. The lungs were œdematous and the liver was fatty.

This is an example of extensive parenchymatous changes in the kidneys, with little obliterating change in the vessels, associated with dilatation of the heart cavities and degeneration of the ventricular walls, and slight atheromatous changes in the coronary arteries. The age and grossly intemperate habits of the patient were strong predisposing causes to the cardiac degeneration, but the extensive cardiac dilatation which existed was evidently due to the intraventricular blood pressure which is a part of the closing history of such renal changes.

CASE V.—My fifth observation was made in a merchant, sixty-eight years of age, a descendant of a long-lived race, with no hereditary taint except gout. Fifteen years ago he came under my medical care with his first attack of gout; from that time he had periodical gouty attacks two or three times each year. Crippling of the small joints developed slowly, and was never excessive. He accustomed himself to an active out-of-door life until two years before his death. So long as he did he showed an unusual physical vigor. During the last four years of his life the signs of cardiac enlargement were slow but progressive. At no time, either by the pulse or sphygmographic tracings, was I able to detect any evidence of increased arterial tension except during and just preceding his gouty attacks.

Early last summer, without any apparent cause, he began to lose weight, became pale, was troubled with insomnia, and had dyspeptic symptoms. His urine increased to sixty and eighty ounces in the twenty-four hours, was of low specific gravity (1008 and 1010), and contained a trace of albumin and hyaline casts. This was the first that I had ever been able to find albumin in his urine. In September he had a mild attack of gout which confined him to his room for a longer period than usual, after which he occasionally had attacks of difficult breathing.

On the 1st of October he came into my office complaining of shortness of breath and pain in his chest. He said that he found it very difficult to walk from his carriage up my steps. I had not seen him for three months, and was surprised at the change that had occurred in him. He was pale and emaciated, and his countenance had a tired expression. His respirations were labored and irregular; his pulse was feeble, irregular in force, and at times intermitted; his heart impulse was feeble and indistinct. The area of cardiac dulness extended from an inch to the right to three inches to the left of the sternum. There was a distinct epigastric impulse. The first sound of the heart was indistinct, and was accompanied by a blowing murmur which was not distinctly audible beyond the ventricular area; the second sound was accentuated, and the cardiac rhythm was irregular. The area of hepatic dulness was increased two inches beyond its normal limits. There was œdema of the feet and ankles, and at the base of both lungs. His urine contained fifteen per cent. of albumin, hyaline and granular casts. He passed

from twenty to forty ounces in twenty-four hours. From this time he rapidly became worse, and in January died in a condition of general anasarca.

At the *autopsy* the lungs were engorged and œdematous. The liver was large and fatty and showed interstitial changes. The heart weighed eighteen ounces, was flabby, and its cavities were all dilated, especially the right ventricle. The anterior wall of the left ventricle was three-fourths of an inch in thickness. The cut surfaces of both ventricles presented a yellow hue, were soft and flabby. Microscopical sections from different portions of their walls showed the muscular fibres fatty; the transverse striæ were almost obliterated; the connective tissue was uniformly increased. The aorta was atheromatous, and the valves, excepting a few patches of atheroma, presented a normal appearance. Atheromatous patches were scattered along the line of the coronary arteries and the calibre of these vessels was somewhat diminished. The kidneys were small, weighing five ounces and a half, and presented the gross and microscopical evidences of small granular kidneys.

This is an example of extreme cardiac dilatation, accompanied by fatty, granular, and connective-tissue changes in the cardiac walls, associated with a small granular kidney. As we analyze this case, the first cardiac change was unquestionably hypertrophy, which for a long period was purely compensatory; but when the glandular structure of the kidneys became involved, degenerative changes in the heart muscle led to extensive dilatation and heart insufficiency, which were followed by the train of symptoms which are so often present in this class of cases.

CASE VI.—My sixth observation was made in a chemist, forty-two years of age, who had had three severe attacks of acute rheumatism before he was twenty years of age. I first examined him in March, 1883, when he consulted me for dyspepsia. On examination, I found a forcible apex-beat two inches below and to the left of the left nipple, with a double aortic murmur at the base of the heart which was transmitted into the vessels and downward to the xiphoid cartilage. He gave no cardiac symptoms except a forcible heart action when excited, and occasional attacks of vertigo. Repeated examinations of his urine gave negative results. By a carefully regulated life, a simple diet, and ten drops of the tincture of digitalis twice a day, he was relieved of his dyspeptic symptoms, and after a few weeks passed from my observation.

In January, 1886, I saw him again, when he was suffering from extreme dyspnœa and had considerable œdema of his lower extremities. I found the apex beat less forcible and further to the left than at my previous examination. There was a distinct epigastric impulse, with marked pulsation of the carotids and subclavian vessels. A blowing, systolic murmur was heard at the apex and transmitted to the left, and a loud gushing diastolic murmur was heard at the base and transmitted along the vessels. Any sudden exertion

caused giddiness and faintness. There were fine moist râles at the base of both lungs, and he was unable to sleep except in a sitting posture. His urine was scanty, high-colored, with specific gravity varying from 1015 to 1024, contained ten per cent. of albumin, hyaline and granular casts. He steadily grew worse, his dyspnœa becoming more constant and severe, and, in a condition of general anasarca, he died from heart insufficiency, on the 6th of the following March.

At the *autopsy* the aortic valves were found thickened, contracted, and fringed with warty excrescences; the anterior leaf of the valve was ruptured and the ruptured edge was studded with recent vegetations. The mitral orifice was surrounded with vegetations; the valves were thickened, and presented a funnel-shaped prolongation on their auricular surface. One of the chordæ tendineæ was broken across at its valvular attachment. The heart weighed twenty-two ounces and a half. The walls of the left ventricle and auricle were thickened and showed evidences of interstitial myocarditis. All the cardiac cavities were dilated, especially the right ventricle. The lower portion of the right lung was the seat of cardiac pneumonia. The liver was enlarged and firm. The kidneys were of a pale-red color and weighed sixteen ounces; their surface was smooth, their capsules slightly thickened, but not adherent. On section, the cortex was thickened, the markings distinct, the pyramids of an unusually dark color. The cortex tubes were dilated, their epithelium swollen, granular, and detached. The arteries of the medullary and cortical substance were enlarged and thickened, and there was an extensive growth of new tissue diffused through the cortex.

This is an example of extensive valvular disease with hypertrophic dilatation of the heart associated with large white kidneys. From the history it seems evident that the kidney changes were secondary to the cardiac. The enlargement and thickening of the renal arteries would be sufficient to suggest such a connection without the history.

In connection with the foregoing observations, I have endeavored to present, in tabulated form, the renal, cardiac, and vascular changes which were found at the autopsies of forty cases of chronic Bright's disease.

In these tables will be found most of the varieties of kidney lesions usually present in this disease. Twelve were studied as private patients during a considerable period before death, with a definite purpose of endeavoring to ascertain the relationship, if any exist, between the renal and cardiac changes. The remainder were hospital patients, the records of which were made under my direction. All examinations were made in the same way, and a uniform method was pursued in preparing and staining the microscopical sections. In eight cases a microscopical examination of the kidneys, heart, and vessels of the

brain was made. In thirty-two cases the examination was limited to the kidneys and heart. An analysis of these cases readily groups them, according to features described more or less minutely, as follows:

1. In eleven cases the vaso-renal changes seemed confined to the adventitia. It is true that frequently no sharp line of demarcation can be demonstrated between the external coat of the vessels and the interstitial sustentacular tissue of organs, and this is especially true of the *arcade arteries* of the kidney. The smaller vascular branches do not present the same difficulty. No case has been placed under this head except those in which the line appears well established in consequence of the difference in the age of the respective tissues.

2. In twelve cases the intima alone was involved.

3. Especial reference has been made in five instances of thickening of the muscular media of the smaller vessels or renal arterioles.

4. In six cases the vascular lesions were not accompanied by changes in the glandular structure of the kidneys.

5. In four cases there were the tubular changes of parenchymatous nephritis with little change in the vessels.

6. In ten cases the changes were mainly in the connective tissue stroma and the adventitia of the vessels.

7. There were twenty-four cases in which the cardiac walls were thickened, and twenty-nine in which the heart muscle had undergone some form of degeneration; three cases exhibited no degenerative change in the cardiac muscle in connection with hypertrophy of the cardiac walls. In these cases the renal changes were confined to the vessels.

8. In twenty cases the heart muscle showed degenerative changes accompanying the hypertrophy. In these cases tubular and stroma changes in the kidney were associated with obliterating changes in the vessels.

9. In four cases, cardiac dilatation and degeneration were accompanied by marked thickening of the cardiac walls.

10. In seven cases the heart was not hypertrophied, or the walls but slightly thickened, notwithstanding there were extensive obliterating vascular changes in the kidneys.

11. Of the twenty-nine cases of fatty and granular degeneration of the cardiac muscle, twenty-seven showed degeneration of the renal epithelium.

TABLE OF KIDNEY, CARDIAC, AND VASCULAR CHANGES IN CHRONIC BRIGHT'S DISEASE.[1]

Name	Age.	Habits.	Duration of disease.	Mode of death.	KIDNEY.			HEART.					VASCULAR, ETC.	
					Wt.	Macroscopical.	Microscopical.	Wt.	Measure.	Cavities and valves.	Coronary art.	Microscopical.	Aorta.	Lungs, etc.
James McH. (1)	35	Hard drinker; rheumatism one year ago; lead colic.	18 mos	Coma; meningitis.	Right 4 ozs.	Capsule thickened and adherent; surface granular; cort'x diminished in thickness; markings absent.	Epithelium of tubes granulo-fatty, most marked in convoluted portion; tubes dilated and tortuous; connective tissue increased; some glomeruli show amyloid degeneration.	14 ozs.	Right ventricle, 1/8 in.; left ventricle, 3/4 in.	Cavities somewhat dilated; aortic valves thickened and slightly incompetent.	Normal.	Muscular fibre normal in size, but granular; nuclei indistinct.		
Harriet W. (2)	39	Unknown.	Exact period not known	Sudden	Both, 6 ozs.	Color deep red; capsule slightly thickened, and, at points, adherent; surface smooth; cortex normal thickness; markings distinct.	Slight degeneration of the tubular cells; round-celled infiltration, about glomeruli particularly; general passive congestion.	18 ozs.	Rt. v., 1/4 in; left v., 1 in.	Cavities somewhat dilated, excepting left auricle; surface showed evidence of old pericarditis; slight atheroma of mitral; endocardium of ventricles opaque; col. car. fibrous.	Thickened walls and diminished lumen.	Transverse markings deficient; nuclei obscured; fibres granular and fatty; great increase of fibroid material, showin places as a dense network of fibrous tissue.	Atheromatous patches	Lungs slightly emphysematous; old pleuritic thickenings and adhesions over both lungs.
John M. (3)	48	Not known; claimed to have been a moderate drinker.	3 years out of health; anasarca 6 weeks before death.	Coma.	Both, 6 1/2 ozs.	Reduced in size; surface uneven; capsule slightly thickened, and adherent; color pale; cortex increased in thickness, while medulla is diminished.	Malpighian capsules filled with amyloid material; stroma slightly increased; interlobular and arcade vessels amyloid.	12 1/2 ozs	Rt. v., 3/32 in.; left v., 1/2 in., covered with fat.	Cavities not markedly dilated; valves competent.	Normal.	Muscular tissue normal.	Aorta dilated, admitting three fingers.	Lungs œdematous.

[1] The measurements for thickness of the cardiac ventricular walls have been taken anteriorly, just below the auriculo-ventricular groove. The tissue in every instance has been hardened in Müller's fluid followed by alcohol.

Martha L. (4)	20	Temperate; gouty.	1 year.	Convulsions and coma; anasarca.	Both, 5½ ozs.	Size below normal; lobulated on surface; capsule markedly thickened.	Bowman's capsules thickened, and contained amyloid material; medium-sized arteries thickened; collecting-tubes contained hyaline material.	17 ozs.	Left v., ⅞ in.	No dilatation of cavities; valves competent though edge of mitral slightly thickened; acute endocarditis.	Normal.	Muscular fibre of left ventricle increased in size.	Aorta below normal size.	Lungs engorged and œdematous.
Catherine R. (5)	66	Temperate	2 years.	Pneumonia, heart failure, œdema feet and ankles.	Both, 7 ozs.	Capsule thickened and adherent; surface granular; color deep red; cortex diminished in thickness.	Stroma increased, mostly near capsule; epithelium of proximal convoluted tubes in condition of granular degeneration; outer walls of vessels thickened.	13 ozs.	Rt. v, ⅛ in, left v., ¾ in.	Ventricular cavities greatly dilated; the left most marked; tricuspid thickened.	Atheromatous; lumen diminished.	Muscular fibre granular and fatty, and contained brown pigment; connective tissue increased.	Atheromatous walls thickened.	Upper half of right lung in 2d stage of acute lobar pneumonia; liver atrophied and fatty
John E. (6)	49	Grossly intemperate.	1 year.	Found dead in bed.	Rt., 4 ozs.	Pale; capsule non-adherent; cortex diminished; markings poorly shown; medulla engorged.	Advanced parenchymatous degeneration, with small amt. only of con. tissue increase; all arteries above interlobular in size oblit. endart.; thickening of intima, particularly in smaller vessels.	13⅞ ozs.	Left v., ¾ in.	All cavities dilated; all valves with atheromatous patches.	Atheroma.	Muscular fibre fatty, with intermuscular connective tissue increase.	Atheroma (slight).	
James W. (7)	38	Temperate.	3 mos.	Convulsions.	Both, 6½ ozs.	Capsule strongly adherent; cortex diminished; markings indistinct.	Tubular epithelium extensively degenerated; intertubular tissue increased; bloodvessels thickened; adventitia.	14 ozs.	Rt. v., ¼ in.; left v. ⅝ in.	Cavities slightly dilated; columnæ carneæ fibrous.	Walls apparently thickened.	Interstitial myocarditis; muscular fibre atrophied.	Walls thickened; calibre diminished.	Lungs engorged with blood, and œdamatous; con. tissue increase in the liver.
Dennis M. (8)	39	Moderate drinker; one attack of rheumatism one year ago.	18 w'ks.	Right side, ple'risy; sudden; of heart failure.	Both, 5½ ozs.	Capsule adherent; cortex diminished; markings indistinct	Epithelium of tubes granular, with desquamation in places; hyaline plugs in many convoluted tubes; connective tissue increased; round-celled infiltration; arterioles showing obliterating endarteritis	16 ozs.	Rt. v, ¾ in.; lt. v., ⅞ in.; old pericard'l adhesions.	Right ventricle dilated; left ventricle, normal; valves competent.	Walls thickened.	Fibre small; striæ indistinct; nuclei obscured; adipose between fibres in abnormal amount.	Not included in autopsy record.	Left pleural cavity filled with fluid; right lung œdematous; liver fatty.
Patrick C. (9)	50	Hard drinker; no syphilitic taint.	1 year.	General anasarca; pulmonary œdema.	Both, 7 ozs.	Surface granular; capsule adherent and thickened; cortex diminished; markings irregular; color pale.	Connective tissue slightly increased beneath capsule; parenchyma in advanced condition of degeneration; arterioles showed obliterating endarteritis—a few were amyloid.	13 ozs.	Rt. v, ⅛ in., lt. v, ⅝ in.	All cavities dilated: slight atheroma of aortic semilunar.	Walls thickened; calibre diminished.	Muscular fibre diminished in size, striæ faint; connective tissue slightly increased; much adipose between bundles.	Cerebral arteries atheromatous.	Liver cirrhotic; fibrous stroma in spleen increased.

TABLE—*Continued.*

Name	Age.	Habits.	Duration of disease.	Mode of death.	KIDNEY.			HEART.					VASCULAR, ETC.	
					Wt.	Macroscopical.	Microscopical.	Wt.	Measure.	Cavities and valves.	Coronary art	Microscopical.	Aorta.	Lungs, etc.
John N. (10)	52	Moderate drinker.	6 weeks last illness; total durat'n not known.	General anasarca; sudden heart failure.	Both, 8 ozs.	Surface smooth and mottled; capsule non-adherent, excepting at a few points, and not thickened.	Stroma normal, excepting beneath capsule, where it was slightly increased; epithelium degenerated; granular casts in tubes; vessels normal.	16 ozs	Rt v., ⅛ in.; lt. v., ⅛ in.	Cavities much dilated; aortic valves thickened and slightly inefficient.	Normal	Fibres atroph'd; nuclei obscured; striæ indistinct; connective tissue increased.	Normal.	Left, pleuropneumonia; liver, passive hyperæmia.
Marcus D. (11)	22	Temperate.	9 weeks (last illness).	General anasarca.	Both, 7 ozs.	Color dark; surface smooth; cortex thickened; markings distinct.	Round-celled infiltration about Malpighian bodies; epithelium in advanced state of granulo-fatty degeneration; extravasation into collecting tubes at points.	10 ozs.	Normal	Both normal.	Normal.	Muscular fibre atrophied; striæ indistinct; nuclei prominent; increase of intermuscular connect. tissue.	Normal.	Liver, marked passive congestion.
Martin B. (12)	66	Temperate.	2 years	10 days previous to death became hemiplegic; coma.	Both, 6½ ozs.	Capsule thickened and adherent; color dark red; cortex greatly diminished.	Cortical tubules largely obliterated by connective tissue; bloodvessels show thickened walls and diminished, often obliterated, lumen.	10 ozs.	Rt. v., ⅛ in., lt. v., ½ in.	Cavities greatly dilated; organs soft and flabby; valves showed spots of atheroma.	Atheromatous at points nearly obliterating lumen	Muscular fibre of left ventricle shows rows of oil globules, instead of normal parenchyma; striæ invisible.	Aorta atheromatous and slightly dilated.	Lungs engorged; liver enlarged, fatty; brain not exposed
John O'N. (13)	41	Intemperate.	Well until three months ago; œdema of feet.	Sudden; coma.	Both, 7 ozs.	Somewhat enlarged; color dark-red; surface smooth.	Arteries showed extensive obliterating endarteritis; parenchyma and stroma normal.	18 ozs.	Rt. v., ⅛ in.; lt. v., 1¼ in.	Both normal.	Slight thickening.	Muscular fibre increased in diameter and granular; striæ not demonstrable; nuclei large and granular; some intermuscular fat	All large bloodvessels thickened.	Lungs engorged and œdematous, liver slightly cirrhotic; brain not examined.
Julia E M. (14)	38	Temperate.	18 mos.	Sudden œdema of lower limbs.	Right 3½ ozs.	Pale; capsule non-adherent; cortex dim.; markings lost; pyramids congested at their base.	Parenchyma in advanced granulo-fatty change, with fibrous tissue increase; small and medium vessels obliterated by increase of intima.	13¼ ozs	Lt. v., ⅜ in.	All cavities dilated; slight vegetations upon semilunar valves.	Atheromatous.	Muscular fibre small; nuclei poorly shown; striæ very indistinct; much intermuscular fat.	Slight atheroma.	Spleen presented large infarcts.

James H. (15)	44	Intemperate.	First symptom noticed by patient five weeks before death.	Syncope on rising; general anasarca.	Right 2 ozs.; left, 4½ ozs.	Left kidney, surf'ce smooth; capsule slightly thickened; cortex diminished; markings indistinct; right resembles the left in general appearance.	Advanced granular and fatty change in tubules; stroma slightly increased; vessels showed obliterating changes.	17½ ozs.	Rt. v., ¼ in.; lt. v., 1⅛ in.	Tricuspid orifice admits four fingers; valves normal	Thickened; lumen diminished.	Muscular fibres increased in size; striæ lost; nuclei indistinct; diffuse connective tissue increase.	Atheroma in small patches only.	Liver cirrhotic; brain not examined.
Thos. K. (16)	43	Temperate.	Last illness 4 weeks.	Gang'ne of left thigh; general anasarca; coma.	Right 5½ ozs.	Capsule non-adherent; surface smooth and very pale yellow.	Tubes generally denuded of epithelium; connective tissue normal	14½ ozs.	L'rge; walls thin; no meas. taken.	Cavities much dilated; valves normal	Normal.	Fibre in advanced and typical fatty degeneration.	Normal.	Serous cavities all contain'd much fluid; liver fatty.
John M. (17)	46	Moderate drinker.	6 mos.	Sudden slight hemiplegia.	Right 4⅛ ozs.	Capsule adherent; markings normal.	Tubular epithelium normal; slight round-celled infiltration in places; endarteritis obliteration very general.	17 ozs.	Rt. v., ¼ in., lt. v., 1 in.	No changes in cavities or valves.	Normal.	Muscular fibre increased in size; nuclei large.	No change in large vessels; small arteries show atheroma.	Blood-clot in right lateral cerebral ventricle.
Wm. O. (18)	47	Intemperate.	1 week.	Acute alcoholism; collapse.	Right 4¼ ozs.	Capsule non adherent; cortex slightly diminished; markings fairly shown.	Epithelium normal; connective tissue increased around small arteries only.	8½ ozs.	Lt. v., ½ in.	Left ventricle greatly dilated; valves normal.	Patches of atheroma.	Slight pigmentation of muscular fibres.	Patches of fatty change.	Lungs, acute mil. tuberculosis; passive congestion of liver
Clara L. (19)	35	Intemperate.	Unknown.	Uræmia coma.	Both, 14 ozs.	Greatly enlarged; capsule adher'nt; cortex increased; markings obliterated; intense engorgement.	Tubes largely denuded of their lining cells; round-celled infiltration; no arterial change.	14 ozs.	Lt. v., ½ in.	Ventricles dilated; valves normal.	Atheromatous patches.	Muscular fibres normal.	Normal.	Liver hypertrophic; cirrhosis.
John S. (20)	36	Temperate.	Unknown.	Asthenia.	Right 4½ ozs.	Cortex diminished; markings indistinct.	Free borders of cells denuded; no other changes.	10 ozs.	Not meas. no abnorm. obeserved.	Both normal.	Normal.	Normal.	Normal.	Lungs, fibroid phthisis.
James W (21)	38	Intemperate.	1 year.	Sudden; under ether.	Right 4 ozs.	Normal appearance.	External coat of small arteries markedly thickened.	9 ozs.	Lt. v., ½ in.	Left ventricle dilated; valves competent.	Minute br'nch's atheromatous.	Extensive interstitial myocarditis.	Atheroma.	Abscess of liver; dysenteric ulcers.

TABLE—*Continued.*

Name	Age.	Habits.	Duration of disease.	Mode of death.	KIDNEY.			HEART.					VASCULAR, ETC.	
					Wt.	Macroscopical.	Microscopical.	Wt.	Measure.	Cavities and valves.	Coronary art.	Microscopical.	Aorta.	Lungs, etc.
Sarah H. (22)	23	Temperate.	18 mos.	Asthenia.	Right 5 ozs.	Markings indistinct; capsule adherent; cortex thickened.	Fatty degeneration of tubular cells, particularly borders of those lining convoluted tubes	8½ ozs.	Lt. v., ½ in.	All cavities dilated; valves competent.	Patches of fatty degeneration.	Well-marked fatty degeneration of muscular fibre.	Normal.	Acute mil. tuberculosis both lungs; right, pleurisy; old.
Sam'l S. (23)	45	Intemperate.	Unknown.	Coroner's case.	Right 4½ ozs.	Cortex diminished; capsule adher'nt; markings indistinct.	Two tubular cells remaining; connect. tis. increased; exter'l walls of arteries thickened	20 ozs.	Lt. v., ⅝ in.	Left v. extensively dilat'd; mitral atheromatous.	Atheroma.	Fibres atrophied and, in places, granular.	Atheroma.	Liver fatty.
James D. (24)	42	Intemperate.	Unknown.	Sudden; on stre't coroner's case.	Right 4½ ozs.	Capsule slightly adherent; cortex diminished; markings indistinct.	Con'tive tissue increased especially around tufts and small arteries; no change in tubular epithellium,	10 ozs.	Lt. v., 1 in.	No dilatation of cavities; valves competent.	Normal.	Fibres hypertrophied; otherwise normal.	Normal.	Hæmatoma of brain.
Sarah U. (25)	30	Temperate.	Last sickness 6 mos.	Coma.	Right 3⅞ ozs.	Capsule adherent in patches; cortex slightly diminished; markings indist.	Thickening of adventitia of small arteries; no parenchymatous degeneration.	9 ozs.	Lt. v., ½ in.	Lt. v. small; valves all competent.	Athermatous patches.	Fatty degeneration of muscular fibre.	Slight atheroma.	Œdematous.
James C. (26)	44	Generous liver; gouty.	2 years.	Coma.	Right 5 ozs.	Dark-red, smooth capsule non-adherent; cortex slightly thickened; markings distinct	Slight round-celled infiltration about glomeruli; vessels in state of oblit. endarteritis.	19 ozs.	Lt. v., 1¼ in.	Normal.	Normal.	Fibres coarse; striæ somewhat indistinct.	Normal.	Obl. changes in cerebral vessels; clot in both lateral ventricles.
Wm. S. (27)	45	Moderate drinker.	1 year.	Sudden; hemiplegia.	Right 5¼ ozs.	Surface smooth; capsule non-adherent; markings indistinct.	Parenchyma normal; few waxy tufts; slight round-celled infiltration; marked obliteration of arteries by thickening of intima.	18¼ ozs.	Lt. v., 1⅛ in.	Cavities generally dilated and left v. markedly increased; valves competent.	Normal.	Fibres much increased in size; striæ sharp and distinct; nuclei large.	Atheroma (slight).	Blood-clot in left lat. ventricle; cere. ves. generally seat of obliterating changes.
H'nry R. (28)	37	Moderate drinker; history of gout.	13 mos.	Sudden; general anasarca.	Right 7 ozs.	Smooth; capsule non-adherent to cortex; pale and thickened.	Parenchyma granular and often detached; slight round-celled infiltration; lumen of arterioles diminished.	20 ozs.	Lt. v., 1 in.	Cavities all dilated.	Atheromatous.	Fibres normal size; striæ distinct; nuclei granular; con. tis. increased.	Atheroma.	Cerebral arteries greatly thicken'd (oblit. endart.).

Richard B (29)	68	Intemperate.	4 years; gouty attacks for 15 years.	Coma; pulmonary œdema.	Right 3½ ozs.	Small; capsule adherent; cortex diminished, surface granular.	Larger arteries showed thickened intima and adventitia; advanced parench. degen.; c. tis. betw. tube increased.	11 ozs.	Lt. v., ⅝ in.	Cavities all dilated.	Atheroma; calibre diminished.	Fibres granular and fatty; striæ and nuclei indistinct; connect. tis. increased.	Atheroma.	Liver fatty and cirrhotic.
John S.W. (30)	53	Intemperate.	10 yrs.	Sudden; pulmonary œdema	Right 7 ozs	Surf. smooth; color deep red; old and recent infarcts; markings norm.	Parenchyma granular; increase of middle coat of small arteries.	23 ozs.	Lt. v., ¾ in.	Cavities all dilated.	Atheroma (slight).	Musc. in mark'd degree of fatty degeneration.	Atheroma (slight).	Infarct in lower lobe of both lungs
John, M. (31)	42	Temperate.	Acute rheumatism 10 yrs. ago.	Sudden.	Both, kidneys weigh 9¼ ozs.	Surface smooth; capsule slightly thickened, but non-adherent; cortex thickened; markings distinct; pyramids dark.	Tubes dilated; epithelium swollen, granular, and in places detached; arterioles show thickened walls; connective tissue generally increased.	20½ ozs.	Lt. v., ¾ in	Cavities all dilated, and especially the rt. v.; aortic vals. fringed with warty excres.; ant. leaflet rupt.; mitral opening sur. with veg.; 1 of the chordæ tend. broken; all valves mark. thick.	.....	Intermuscular connective tissue increased; muscular fibres in condition of fatty degeneration.	.....	Liver: passive hyperæmia. Lungs: brown induration and œdema.
Jennie S. (32)	12	Temperate.	14 mos. scarlet fever 18 mos. ago.	Sudden; diabetic coma.	Right 3½ ozs.	Surface granular; color pale (mottled) red; pyramids engorged at base.	Marked round-celled infiltration; parenchyma granulo-fatty, with free borders generally wanting; oblit. of arteries by thickening of intima	6 ozs.	Lt. v., ⅜ in.	Cavities all dilated; valves competent.	Normal	Muscle fatty; nuclei faintly shown; striæ almost obliterated.	Normal.	Pulmonary œdema; diabetes.
Thos. P. (33)	40	Intemperate.	1 year; history of heart disease.	Coma.	Right 4¾ ozs.	Capsule adherent; surface pale, granular; cortex diminished; markings indistinct.	Tubules of cortex dilated and tortuous, with granulo-fatty cells often detached and forming plugs in lumen; connective tissue greatly increased; intima and adventitia of vessels increased.	17 ozs.	Lt. v., 7/8 in.	Cavities all dilated; valves competent, excepting for two fenestræ in one cusp of aortic valve.	Normal as to size and gross appearance.	Muscular substance showed fatty degeneration.	Normal.	Pulmonary engorgement.
Benjamin S. (34)	41	Grossly intemperate.	2 years.	Coma.	Right 5¼ ozs.	Color intense red; capsule thickened and adherent, excepting at a few points.	Markings fair, excepting when bands of connect. tissue pass from capsule to pelvis; parenchyma extensively granulo-fatty altho' cells all intact; muscular media of large arteries increas'd and amyloid; many glomeruli, also waxy.	12½ ozs.	Lt. v., ½ in.	Cavities all increased; valves competent; very slight roughening of endocardium covering aortic cusps.	Lumen diminished.	Muscular fibre granular; nuclei and striæ indistinct.	Normal.	Right lung solid from lobar pneumonia.

TABLE—*Concluded.*

Name	Age.	Habits.	Duration of disease.	Mode of death.	KIDNEY.			HEART.					VASCULAR, ETC.	
					Wt.	Macroscopical.	Microscopical.	Wt.	Measure.	Cavities and valves.	Coronary art.	Microscopical.	Aorta.	Lungs, etc.
Anson W. (35)	43	Intemperate at rare intervals only.	10 mos.	Convulsions after exposure during spree	Right 5½ ozs.	Color red; surface uneven; capsule thickened and adherent; cortex diminished.	Extens. degen. of tubular cells; many tubes oblit.; connective tissue much increased; many tufts waxy; external coat of arteries thickened.	16 ozs.	Lt. v, 1⅞ in	Cavities much dilated; valves normal.	Normal.	Connective tissue much increased; fatty degeneration of muscular fibres.	Dilated.	
Freeman H. (36)	56	Grossly intemperate.	4 years.	Coma.	Right 7¾ ozs	Surface smooth; capsule adherent, slightly; cortex much thickened; markings indistinct.	Tubules very generally denuded of cells; connective tissue greatly increased; external coat of arteries increased.	20 ozs.	Lt. v, ⅝ in	Cavities dilated, particularly the right vent.; valves competent.	Atheroma; calibre diminished	Muscular fibre granular and atrophied.	Dilated.	Lungs œdematous.
Julius P. (37)	39	Temperate.	15 mos.	Convulsions; œdema of legs.	Right 6¼ ozs	Surface granular; pale; cortex increased; capsule non-adherent; markings fair; base of Malpigh. pyramid engorged with blood.	Convoluted tubes dilated, epithel. almost all wanting; extensive round-celled infiltration; blood in collecting-tubes; external coat of large arteries much thickened.	13½ ozs	Lt. v, ⅜ in.	Cavities all dilated; valves all competent.	Atheroma.	Fibres small; nuclei clear; protoplasm granulo-fatty.	Atheroma.	Atheroma of basilar artery.
Adam B. (38)	47	Intemperate.	2 years.	Sudden.	Both, 14 ozs.	Cortex slightly diminish'd; capsule adherent; markings indistinct; color deep red.	Connective tissue increased about tufts, and around small arteries; parenchymatous degeneration.	20 ozs.	Lt. v., ⅜ in.	Left ventricle dilated; valves normal.	Normal.	Muscular fibre in condit'n of fatty degener'n, not evenly distributed, however.	Atheroma.	Liver: hypertrophic cirrhosis.
Thos. C. (39)	45	Always been a "before breakfast drinker."	5 mos.	Sudden.	Both, 9 ozs.	Small; color red; cortex diminished; capsule adherent; mark'gs poorly shown.	Interstitial and parenchymatous nephritis; obliterating endarteritis well marked.	8¾ ozs.	Lt. v., ½ in	Left ventricle dilated; valves normal.	Atheroma.	Intestinal myocarditis, with fatty degeneration.	Atheroma.	Pulmonary œdema; liver cirrhotic.
Jenerva A. (40)	42	Temperate.	2½ yrs.	Coma.	Right 3 ozs.	Small; deep red; cortex dimin.; fibrous bands projected from capsule; small infarcts throughout	Excreting epithelium largely absent; connective tissue much increased, with thickening of outer coat of arteries; small arteries amyloid.	11 ozs.	Lt. v., ⅝ in.	Left ventricle dilated; valves competent; abscess in anterior wall of left ventricle.	Atheromatous, with many sm. extravasa.	Slight increase of con. tissue; mus. fibre large and granular; nuclei prominent.	Atheroma.	Inactive tubercular nodule at right pulmonary apex.

12. Of the twenty-four cases of cardiac hypertrophy, twenty-two showed obliterating changes in the renal vessels.

13. The arteries in the forty cases showed in thirty instances some form of obliterating change.

14. In twenty-two cases obliterating change in the arteries was associated with renal epithelial degeneration.

15. Granulo-fatty degeneration of the heart is found in twenty-nine instances. In twenty-four this is accompanied by obliterating changes in the renal vessels.

16. Of the twenty-four cases of cardiac hypertrophy, twenty are accompanied with degeneration of the parenchyma of the kidneys.

A general review of my cases fairly indicates the following conclusions:

As a rule, the more extensive the obliterating changes in the renal arterioles, the greater the degree of cardiac hypertrophy. I have been impressed by the distinct relations which one seems to bear to the other. In the few cases in which cardiac hypertrophy was not present with the obliterating changes in the arteries, the general nutritive condition was markedly faulty. In two instances of drunkards, although the kidneys were extensively granular and the arteries much thickened, no evidence of hypertrophy existed, although the dilatation was excessive. Even in the cirrhotic kidney the primary cardiac hypertrophy has no necessary connection as cause and effect with the kidney changes, but both are a part of the general fibrosis which has its origin in a fibroid diathesis, either hereditary or acquired, the hereditary influences being far stronger than the acquired. In two cases in which the cardiac hypertrophy was most extensive, the only change found in the kidneys was obliterating arteritis in the renal vessels. In other varieties of Bright's disease the heart changes seemed to be directly dependent upon the renal disease, or rather upon the toxæmia and faulty nutrition which result from them; the evidence of this faulty nutrition was shown first and most markedly by changes in the heart walls and cavities.

In all forms of chronic Bright's disease increased arterial tension and more or less compensating hypertrophy are early phenomena. In the cirrhotic it is usually gradual in its development, in some cases extending over years; but in all instances, when the period is reached at which the renal symptoms clearly establish interference with the func-

tioning anatomical elements of the kidney, a feeble heart action and diminished arterial tension will indicate degenerative cardiac changes. When once the period of tension is passed, it can never be reëstablished, except temporarily by the action of certain drugs.

Synchronous with the failure of heart power, changes occur in the venous blood system which give rise to many of the distressing symptoms which are present in the advanced stage of so many cases of Bright's disease. There seems to be no fixed or uniform relation between the character and extent of the changes in the glandular structure of the kidneys and those in the renal and systemic vessels; the changes observed in the vessels of the pia mater, however, were usually less in degree than those in the kidney. In all cases in which advanced cirrhotic changes in the kidneys existed the ventricular cavities were dilated. In cases of extensive dilatation with little or no thickening of the cardiac walls, some form of degeneration of the muscular tissue existed. If the cardiac substance was soft and flabby, the fibres were granular, their nuclei obscured, and their striæ more or less obliterated, with a loss of consistency which allowed them easily to break down when an attempt was made to separate them. The papillary muscle of the mitral valves was frequently the seat of fibroid change, their columns having lost their fleshy appearance and assumed that of tendinous cords.

The edges of the flaps of the mitral valves were not infrequently found slightly thickened, but the evidences of acute endocarditis, secondary to the renal disease, were present in but one of my cases, while slight thickening and induration at the base and tip of the valves, indicating interstitial endocarditis, were present in five cases.

Cases of sudden heart failure accompanied by dilatation, secondary to cirrhotic kidneys, are sometimes regarded during life as cases of mitral regurgitation on account of the presence of a loud systolic apex murmur; but in no instance did I find at the autopsy evidence of mitral lesions in such cases. Clinical experience teaches that rheumatic valvular disease of the heart may remain latent for many years, during which the heart will perform its functions with efficiency; if, however, renal disease supervene in such a case, its effects are most disastrous, as is evidenced by my fifth case, for the valvular lesions which had remained latent so many years in this case became pronounced under the strain on the circulation which was produced by

the complicating renal disease, and led rapidly to the development of the distressing symptoms of heart insufficiency. The association of valvular affections of the heart with Bright's disease has been sharply insisted upon by many observers. On the one hand, the kidney disease has been claimed to be the cause of the cardiac affection; on the other, valvular lesions have been claimed to be the cause of the renal disease. In only three of my tabulated cases were valvular lesions found which could be regarded as secondary to the kidney disease.

The great discrepancy in the statistics given by different observers in regard to the frequency of the valvular disease as a cause of Bright's kidney is my excuse for presenting the following statistics taken from the records of Bellevue and Roosevelt hospitals.

While it is but fair to admit that hospital records are more or less unsatisfactory when searched with a view of determining any definite point, I think I can justly claim that these records present as fair a basis of comparison as similar reports from other hospitals. The method employed was to take every case of heart disease recorded for a certain period, omitting only those cases of combined cardiac and kidney disease in which there was no evidence of the cardiac lesion having been primary.

A total of one hundred and sixty-four cases was thus gathered. Of this number, forty-three were complicated by evidences of Bright's disease, as shown by the presence of albumin and casts in the urine, or in twenty per cent. of all the cases.

In thirty-five of the forty-three cases there was a history of chronic cardiac disease extending over periods of from one to twenty-five years. Thus, making all due allowances, it seems to me that these cases evidence a much closer relation between valvular disease of the heart and secondary disease of the kidneys than is usually given in hospital statistics.

Three cases included in my tables are well-marked examples of the usual kidney changes found in such cases. In all, the kidneys were enlarged, and, in addition to the usual changes which are found in the large white kidney, the arteries were thickened and dilated; in each of these cases there was right heart insufficiency. The results of these studies seem to indicate that the cardiac changes in chronic Bright's disease have a wide range of variation, and that no one can predict

with certainty the exact nature of the changes which may be present in any given case, except that they probably are degenerative, and that, sooner or later, a condition of heart failure will be reached which may fairly be designated as a Bright's heart. The more marked these are in any case, the more unfavorable the prognosis.

We must, as it seems to me, regard Bright's disease as a constitutional disease, in which the repair and waste in all the tissues of the body are imperfectly carried on, and in one way or another the kidney changes are expressive of other visceral and arterial changes which, combined with kidney lesion, make up the clinical and pathological history of the disease.

DISCUSSION.

DR. FRANCIS DELAFIELD: I wish to draw attention to several of the propositions presented. In the first place, that cardiac hypertrophy is often developed without any changes in the kidneys except thickening of the middle and inner coats of the renal artery. There can be little doubt that disease of the kidney may begin in the arteries of the kidney; but, as a matter of fact, we seldom find such kidneys at the autopsy. We have to look at this question of cardiac hypertrophy with reference not only to the kidneys, but to other parts of the body. In adults and in old persons we may have a growth of connective tissue in the endocardium, in the arteries, in the lungs, and in the kidneys, and this growth of new connective tissue gives us either the character of a chronic inflammation or, as some people call it, a chronic fibroid degeneration of this part of the body. These changes are much more frequently associated together than otherwise. We do not often find a chronic endocarditis alone, a pulmonary emphysema, or chronic diffuse nephritis, yet they can exist by themselves.

Another thing about these lesions is, that they are often borne for years without giving the patient any trouble at all. On the other hand, we know that these lesions in other people are the most fatal ones that can be developed. What is the cause of this difference? Certainly one reason is, a certain number of these patients develop in the course of their disease a complicating condition of high arterial tension, which comes and goes in such a way that we are unable to explain it, except by the existence of temporary changes in the walls of the bloodvessels. We know it is one of the chief reasons some of these patients do well and some do badly. It is in this temporary condition of the bloodvessels we have to look for the hypertrophy of the heart, rather than in changes in the structural walls of the arteries. Dr. Loomis also states that morbid changes in the glandular structure of the kidneys are

usually associated with morbid changes in the cardiac walls, and dilatation of the cardiac cavity. This point of dilatation and degeneration of the heart in connection with Bright's disease seemed to be the principal point of Dr. Loomis's paper. My experience differs from Dr. Loomis's in this respect. For myself, I should say that degeneration and dilatation of the heart without endocarditis, are certainly not the rule in chronic Bright's disease. It is not nearly as common as simple hypertrophy of the left ventricle. In many of these patients, when they do badly, we have the evidence of feeble heart during life, but I do not think, if we examine these hearts after death, we frequently find dilatation or degeneration.

DR. GEORGE L. PEABODY: I wish to allude to one part of the paper in which some observations made by me[1] bear out the statements of Dr. Loomis. Some years ago I reviewed the record of 2000 consecutive autopsies, and though I have not with me the actual figures produced at the time, in a paper subsequently read by me before the Academy of Medicine of New York, I showed the percentage of cases in which the heart was not hypertrophied in well-marked cases of chronic diffuse nephritis. In these 2000 cases the number in which this form of chronic Bright's disease was present was very large, for the reason that in the hospitals of New York they have a large proportion of cases of chronic Bright's. In those cases in which I had recorded notes and made microscopic examination of the kidney, there was a large percentage of them in which there was no hypertrophy. The observations I have made since have borne out the conclusions reached then. I have long since come to the conclusion that, as a means of assisting in the clinical diagnosis of chronic Bright's disease in this country, we have to abandon the clinical evidence of the size of the heart for the reason that the size of the heart does not always vary. These were all hospital cases.

DR. WILLIAM OSLER: I think we might divide the cases of chronic interstitial nephritis into two great groups, as we see them clinically and in the post-mortem room. One, in which we have hypertrophy of the left ventricle. These are the cases which die suddenly with uræmia, apoplexy, or inflammation of serous membranes. They, in my experience, have invariably hypertrophy of the left ventricle. There is another group in which there are marked signs of cardiac failure, often associated with dropsy. These have a dilated heart with degeneration of the muscular substance. I regard the dilatation as secondary, due to the failure in the nutrition of the muscular substance, and in a considerable proportion of cases degeneration is associated with arterio-sclerosis in the coronary arteries.

DR. A. L. LOOMIS: Dr. Delafield's observations must have been made in a different way, or for a different purpose, than those which I have reported in my paper. I have found degeneration of the heart walls in a much larger proportion of my cases than I had been led to expect from previous observations which I had made based on gross appearances only. I certainly did not enter into these investigations with any preconceived views on the

[1] New York Medical Record, vol. xxix. pp. 235–237, 1886.

subject. I do not think that the cardiac degeneration and dilatation are primary, but I wish to convey the idea that in all forms of chronic Bright's disease before the fatal issue is reached, there exists in the majority of cases dilatation of the heart cavities, which is usually accompanied by some form of degeneration of the heart walls, and that the first unfavorable symptom in patients with chronic Bright's disease is heart insufficiency. I do not regard the cardiac hypertrophy as necessarily due to the kidney lesions, but to a general arterial fibrosis. During the period of middle life we most frequently meet such arterial and cardiac changes, and if Bright's disease develops at this period we are likely to have a hypertrophied heart preceding the kidney changes. I would not assert that these degenerative changes are present in all cases of cardiac dilatation, but the microscopical examination of those hearts which were markedly feeble in their action during life showed some form of degeneration of the muscular fibre, which leads me to believe that degeneration may occur only a short time before death. When heart insufficiency is once established in Bright's disease, we must recognize the fact that life cannot be prolonged for any length of time.

CHRONIC INTERSTITIAL NEPHRITIS AND ANGINA PECTORIS.

By SAMUEL C. CHEW, M.D.,

PROFESSOR OF THE PRACTICE OF MEDICINE, UNIVERSITY OF MARYLAND, BALTIMORE.

THE subject which I propose to consider in this paper is the relation between certain symptoms referable to the kidneys, and certain other painful and oppressive symptoms relating to the heart.

The former of these two groups of symptoms consists of the occasional presence of albumin in the urine; the occasional occurrence of tube-casts, commonly of a fine granular or hyaline character; and, in general, a low specific gravity of the urine: signs pointing to that form of Bright's disease which is pathologically a chronic interstitial nephritis. The latter group is made up of a feeling of constriction in the chest more or less constantly present, sometimes in moderate degree and sometimes so greatly intensified as to be an acute agony, or an anguish even deeper than that, "a sense of ruin which is worse than pain, a masterful negation and collapse," a feeling of impending dissolution; the condition, in a word, to which the term angina pectoris is commonly applied, and with the emotional as well as the sensory phenomena belonging to that dire disease.

Each one of these groups of symptoms is, taken by itself, almost a commonplace of medical experience; but their association with each other is perhaps not very frequently met with, and yet must be occasionally encountered by physicians in general practice. How are they correlated with each other? Is the connection merely an incidental one, so that the anginoid symptoms might have occurred in any given case even had there been no renal disease? Is the condition indicated by the signs referable to the kidneys the direct cause of the cardiac symptoms? Or is there anything in the state of a heart laboring under such symptoms which may directly or indirectly determine albuminuria?

It is well known that some forms of Bright's disease are likely to cause, or at least to be associated with organic changes in the heart, and the manner in which such cardiac changes are brought about has been the subject of much ingenious speculation and able argument, as instanced most recently in the thoughtful and suggestive Middleton-Goldsmith lecture of Professor Da Costa.

It is equally true that an organic change in the heart resulting in dilatation of the organ and general venous stasis may determine secondary disease of the kidneys. But the cases which I have in view would not seem to belong to either of these categories. They may occur without any hypertrophy of the heart ascribable to the renal disease, if it be admitted that the latter can cause such cardiac change, and they may occur also without any such dilatation of the heart as could of itself have produced passive renal congestion. Are both groups of symptoms the result of a common cause acting upon both of the involved organs? I ask the question, but I propose to consider the subject chiefly from a clinical point of view, narrating some of the cases in which I have observed these conjoint symptoms, suggesting perhaps some explanation of them, but trusting that they may receive from others a more complete pathological interpretation than I am myself able to offer.

In the first place I would state that, so far as my reading goes, there is no express mention of a relation between these maladies in any systematic treatise upon the subjects.

Heberden, who first described angina pectoris and gave it the name by which it has been known since his day, states that he had seen nearly a hundred persons suffering with the disease, yet he makes no reference to dropsical symptoms as ever attending it. This he would probably have done had he observed them, though, of course, without associating them with kidney disease, as his observations were anterior to the existence of renal pathology.

In Dr. John Blackall's report of cases of angina pectoris appended to his work on *Dropsies*, which was published before the year 1820, he quotes an account of the case of Lord Clarendon, who died of angina pectoris occurring with symptoms which, as Dr. Blackwell says, "rendered probable the existence of some disorder of the urinary organs." These symptoms were great frequency of micturition occurring without pain in the urinary organs themselves, but with intense

pain in the left arm. "The circumstances," says Blackall, "have been scarce heard of before, and the causes are not yet understood by any physician."[1] It would seem not unlikely that in this case the morbid state of the urinary organs unattended with pain was chronic interstitial nephritis, which is so often accompanied with increased frequency of micturition.

To come to a somewhat later time, we find no mention of the combination of the heart and kidney symptoms in Sir John Forbes's masterly article on angina pectoris in the *Cyclopædia of Practical Medicine.*

Professor Gairdner, in his very elaborate and able discussion of angina in Reynolds's *System of Medicine*, merely remarks that "renal disease forms a very serious and often unmanageable complication" of angina;[2] as it certainly does, especially if it give rise to pulmonary œdema; but he does not refer to any pathological connection between the renal and cardiac changes. Nor does any systematic writer, so far as I have been able to ascertain. And yet in several authors there are observations which might have led to the thought of a connection between the two diseases. Thus Gairdner remarks that Lauder Brunton's case, in which the value of nitrite of amyl in angina was first made known, "shows a correlation of some kind between angina-like paroxysms and increased arterial tension;" and it is well known that increased arterial tension is common in Bright's disease. Indeed, Brunton himself, in his statement of the therapeutic applications of nitroglycerine, after referring to its use in angina pectoris, says that "it is useful when the heart is weak, or when the tension is abnormally high, as in Bright's disease."[3]

The consideration of the therapeutic fact that the same agent is beneficial in two diseases, and in the same way apparently—that is, by lessening arterial tension, might suggest a relation of some kind between them. Still nearer the mark is an observation of Strümpell, in his account of the chronic contracted kidney, that "the long-known asthma of renal disease, often termed, without reason, 'uræmic asthma,' does not always have the same origin. It usually depends upon cardiac weakness, and is a genuine cardiac asthma, *corresponding pre-*

[1] Blackall on Dropsies, p. 255.
[2] Reynolds, vol. iv. p. 546.
[3] Brunton's Therapeutics, p. 669.

cisely to angina pectoris in its different symptoms."[1] But he does not attempt to trace a pathological connection between the diseases.

One case of coexistence of angina pectoris and Bright's disease, closely resembling those I have myself observed, is reported by Dr. von Russdorf, of Berlin.[2]

I have selected from my note-book the following cases as illustrative of the combination of diseases to which this paper refers:

CASE I.—G. A., female, seventy-one years old, had for more than a year been failing in strength and appetite, becoming pallid in hue and troubled with a feeling of constriction and oppression in the chest, especially referred to the cardiac region. Occasionally there was slight swelling about the ankles, transient in duration, and disappearing entirely at intervals. On auscultation, there was no evidence of pulmonary trouble of any kind, nor did the heart show any sign of valvular disease, but its action was habitually and markedly weak. The urine showed at times slight traces of albumin, which again would entirely disappear, and there were occasional fine granular and hyaline casts—never abundant, and often absent or not discoverable even when a rigid search was made for them. The specific gravity of the urine ranged from 1012 to 1015. The diagnosis was that of chronic contracted kidney.

The distressing sense of constriction in the chest increased in degree and persistence, until the 23d of March, 1886, when I was urgently summoned to the patient, and found her suffering with agonizing pain in the region of the heart, extending down the left arm. The expression and color of the face were characteristic of angina pectoris. This pain was promptly relieved by a hypodermic injection of morphia; but at various times it occurred, though with less violence than at first, while the sense of constriction and oppression in the chest, which had been felt for several months before the severe attack, became more constant. This form of suffering was, however, controlled to a considerable degree by the administration of nitroglycerine in the dose of $\frac{1}{100}$ of a grain three or four times a day. In order to test the value of this remedy, after administering it for two or three days in succession, and until the patient had been apparently relieved entirely of the constrictive pain in the chest, I would withdraw it, when, in a day or sometimes in two days, she would request its use again. "Give me my little pills," she would say, and again would experience the desired relief from them. She was provided also with pearls of nitrite of amyl in case of the recurrence of an attack of great severity, and occasionally had recourse to them. And so the contest with two mortal maladies went on, in which, in view of the patient's age and the nature of the diseases, little could be done beyond relieving suffering and endeavoring to promote euthanasia, when, on November 19th, or about seven

[1] Strümpell's Text-book, Amer. edit., p. 808.

[2] Deutsche Klinik, vol. xviii. p. 375.

months and a half after the first acute paroxysm, the patient, on walking from her bed to a chair, sank into it and expired.

In this case the symptoms of angina pectoris were classically marked, as were those of Bright's disease in the form, apparently, of the chronic contracted kidney, as shown by the intermittent occurrence of albumin in the urine, the occasional presence and frequent absence of tube-casts, and the small amount of dropsy. There was no opportunity of making a post-mortem examination.

CASE II.—N. B., sixty-four years old, a merchant by occupation, consulted me in November, 1887, for oppressed breathing and a painful sense of constriction in the chest, extending to the left arm, and felt, in general, only when walking, especially when walking up hill or up stairs. Auscultation showed no valvular change in the heart, but its action was weak, and at that time there was no sign of pulmonary disease. Examination of the urine gave a negative response as to the existence of disease in the kidneys, but a few weeks later it showed a cloudy trace of albumin and scanty hyaline and fine granular casts. Later repeated examinations showed sometimes the presence and sometimes the absence of these signs, as is common in chronic interstitial nephritis. The patient was troubled with constipation, and was often imprudent in eating; and the regulation of the bowels, together with restriction for a time to a milk diet, produced a marked improvement in the breathing and a lessening of the constrictive pain in walking. At the same time he was put upon the use of $\frac{1}{10}$ of a grain of arseniate of iron three times a day. In January, 1888, anasarca appeared in the lower limbs as well as slight effusion in the pleural sacs and patches of pulmonary œdema. These symptoms abated under the influence of digitalis and tincture of the chloride of iron.

The weather in Baltimore being at that season very unfavorable for one in the patient's condition, he went, by my advice, to the milder climate of Thomasville, Georgia, where he consulted Professor John T. Metcalfe. This gentleman wrote to me that he detected in the patient's lungs the presence still of some œdema in scattered spots, and he stated that he had frequently met with this patchy sort of œdema in Bright's disease, and thought it somewhat characteristic of the malady. I believe it often is so, and perhaps it is analogous to another localized form of dropsy occurring in the same disease, that, viz., in the lower eyelids. In April the patient returned from Georgia, and in the drier and warmer weather of the spring and summer he has somewhat improved; the effusion in the pleural cavities, the lungs, and the lower extremities has disappeared; but the chemical and microscopical signs of renal disease still exist, and he still suffers occasionally with attacks of angina. On July 19, 1888, albumin was present in the urine to the amount of two per cent. by volume; there were a few hyaline casts, and the specific gravity was

1015. On August 18th, there was three per cent. of albumin by volume, no casts, and a specific gravity of 1011.

In this case, again, the coexistence of the symptoms of angina pectoris and of chronic contracted kidney is plainly shown.

CASE III.—J. W., fifty-three years of age, a physician by profession, was able to appreciate the full significance of all the symptoms belonging to his mortal maladies and to meet them with fortitude. He had been failing in health and growing weak and anæmic for many months, when, in February, 1888, I was invited by his attendant physician to see him in consultation. The most prominent symptoms then existing were great oppression and sense of weight in the cardiac region, with attacks of acute pain in the same situation so severe as to render the hypodermic use of morphia necessary, and also the inhalation of nitrite of amyl, from which temporary relief was gotten. The administration of nitroglycerine was without effect. Dyspnœa was frequently urged, but was not dependent upon any change in the lungs or heart discoverable by auscultation. Anasarca in moderate degree sometimes appeared in the lower limbs, but was transient in duration. The urine was examined many times, often presenting nothing abnormal, but occasionally showing slight cloudiness when tested for albumin, and sparse hyaline casts under the microscope. Yet the presence of these at all, though infrequent, taken in connection with the character of the breathing and the anasarca, pointed to the existence of chronic contracted kidney, while the breast pang showed angina pectoris. No remedies stayed the progress of his organic disease; but little and only temporary relief could be given to the attendant pain and difficult breathing, and on the 8th of April he died.

At the *post-mortem* examination the kidneys were found to be diminished in size, chiefly at the expense of the cortical portion, and their capsules somewhat adherent, though not to so great an extent as is usual in a more advanced stage of atrophic change. The heart was distended by coagula, giving it the appearance of being enlarged before it was opened, but the ventricular walls were attenuated and large fibroid patches were found under the endocardium of the left ventricle. The coronary arteries were both in a state of fibroid degeneration; the right artery being completely occluded by softened fibrous and atheromatous material, and the left so much obstructed that it was only slightly pervious. This degree of obstruction must necessarily have caused great cardiac ischæmia. An aneurismal dilatation of the aorta of the size of a large egg, with atheromatous walls, was found just below the cœliac axis.

Of the three cases here reported, in the first no post-mortem examination could be made. The second is that of a patient still living. In the third the diagnosis of the condition of the kidneys made in life was confirmed by the post-mortem examination, which also showed

changes in the heart not positively diagnosticable, but entirely in accordance with the cardiac symptoms observed, and perfectly explaining them.

It seems a fairly probable inference that inasmuch as Cases I. and II. corresponded in the combination of their symptoms closely with Case III., the same or closely similar changes to those found in Case III. existed in Case I., and exist now in Case II.

There would certainly be no reason for regarding the changes in the heart belonging to Case III., and probably belonging also to Cases I. and II., as in any way causative of the diseased state of the kidneys. And, on the other hand, there would be no reason for regarding the kidney disease in Case III. as productive of the cardiac symptoms through uræmic poisoning or any merely functional disturbance, when positive organic change fully accounting for the cardiac symptoms was found. If uræmia acting on the heart ganglia were the source of trouble in the same way that it occasions such disturbances as convulsions and coma by its influence on other nervous centres, then these heart symptoms might be expected to be as frequent as are the cerebral symptoms of Bright's disease, which they certainly are not. Nor, again, is it easy to see how the kidney disease could have been the direct cause of the organic change in the heart and the symptoms due to it. Bright's disease may, it is true, give rise to endocardial inflammation and to consequent changes in the cardiac valves; but in the cases referred to no such changes existed, and, therefore, the kidney disease could not have held a causal relation to the angina by producing such valvular lesions.

The morbid changes most commonly found in connection with angina pectoris are obstruction of the coronary arteries, insufficiency of the aortic valves, or rigidity of the aorta, possibly interfering with the filling of the coronaries, and fatty or fibroid degeneration of the heart. Any one of these pathological conditions may give rise to cardiac ischæmia, which is most probably the immediate cause on which the angina depends. Cardiac ischæmia is more directly brought about by obstructive disease of the coronaries than by any other lesion, though atheroma or sclerosis of the aorta may accomplish the same result, as may also endo-arteritis or peri-arteritis of the heart arterioles by preventing sufficient blood from entering the myocardium, even though there may be no coronary obstruction. According to the sta-

tistics of Sir John Forbes, coronary disease was found in seventeen out of forty-five cases, or more than one-third.[1] Lussana gives twenty-one cases of coronary obstruction in thirty-six fatal cases of angina;[2] and Liegeois, whose researches are later and probably comprise a larger number of cases, states that three-fourths of all cases may be attributed to atheroma or sclerosis of the coronaries or aorta, or both.[3]

Now all these are degenerative changes, and apparently they cannot be directly attributable to any renal disease. It would seem more probable, therefore, that the change in the heart giving rise to the symptoms of angina is, as is ably maintained by Professor Da Costa in reference to the cardiac hypertrophy of Bright's disease, not the direct consequence of that disease, but a part of a general degenerative change, of which one expression is manifested in the heart and another in the kidney. As Russdorf remarks in the report of his case, "the renal change is the last stage of a slow process of disease, of which the beginning was in the heart."

And, perhaps, if we are inclined to believe with Dr. Da Costa and Dr. Longstreth, that "it is in the nervous system that we are to search for the original source of the malady," we may think that some further probability is lent to this view by the pathological fact of the increased arterial tension found both in Bright's disease and in angina pectoris, and due to a change in the vaso-motor system, and possibly in the cervical ganglion, and the further therapeutic fact that nitroglycerine, which lessens this tension, has proved beneficial in both affections.

DISCUSSION.

DR. GEORGE L. PEABODY: The fact which Dr. Chew brought out at the close of his paper, of the occurrence of angina pectoris without there being recognized after death any vascular lesion, is one of great importance when we consider that a patient may die in such an attack and yet leave no lesion. One clinical fact of importance, that has occurred in my experience, is the frequent failure of the vascular dilators to relieve the paroxysm. Nitrite of

[1] Cyclopædia of Practice of Medicine, vol. i. p. 107.
[2] Virchow's Handbuch, vol. ii. p. 422.
[3] Bull. Méd. des Vosges.

amyl and nitroglycerine are of no value except when there is increased arterial tension. These paroxysms are as likely to come on when there is defective arterial tension as when there is an increase toward the end of life in chronic sufferers from this malady.

DR. S. C. CHEW: It has not been the object of my paper to discuss angina pectoris in all of its aspects, but only in its connection with chronic interstitial nephritis, as illustrated by the cases I have reported.

THE TREATMENT OF VALVULAR DISEASES OF THE HEART.

By J. M. DA COSTA, M.D., LL.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE IN THE JEFFERSON MEDICAL COLLEGE, PHILADELPHIA.

THIS paper is intended to deal with matters connected purely with the treatment of valvular diseases of the heart. In it I desire to record some opinions formed by experience; to trace lines of procedure; to see in how far new therapeutic agents have added to our resources; and to try to make out in what direction the hand of progress points.

Corvisart's famous quotation as a motto for his classical treatise on diseases of the heart, *Hæret lateri lethalis arundo*, represents the thought for a long time prevailing, and still largely adhered to, certainly as regards valvular affections, that nothing can be done for such grave conditions; the fatal arrow must stay implanted until death loosens it. Side by side with this opinion has been, of late years, the doctrine strongly urged that treatment should be based on the particular mischief at valves and valve openings and its supposed necessary consequences on the walls, and rules are laid down for the use or avoidance of remedies in accordance with these preconceived ideas, and with the special name the valvular disease bears. Thus, in mitral regurgitation, as well as, and even more markedly, in mitral narrowing, digitalis is to be employed to increase the propelling power of the heart; in aortic regurgitation, with its large left ventricle, this drug is to be avoided—may be even dangerous; and, as the resulting hypertrophy is mostly sufficient, digitalis is of little or no value in aortic narrowing. In tricuspid regurgitation we are taught by some never to omit its use, whatever else we do; by others, that as there is no compensatory muscular change, it is impossible that it can be of any service.

Now, from these doctrines and the suppositions on which they are based, I dissent. I think they only embody half-truths, which, rigidly applied, will lead to wrong practice. I believe they ought to be taken simply as very general statements concerning the particular tendency of each valve disease, but not be the guide in an individual case without an accurate study of the special features of that case. I hold, then, that the precise valve affected is not, with our present resources, the keynote to the treatment of valvular heart disease. Much more important is it to regard the state of the muscular fibre; the size of the cavities; the condition of the arteries, veins, and capillary system; the secondary results of the cardiac lesion; the control of the nervous system. Important, too, it is to bear in mind the cause of the malady. Holding these views and largely based on them, I advance these propositions for the management of cases of valvular disease of the heart. We are to take as indications:

1. The state of the heart-muscle and of the cavities.
2. The rhythm of the heart-action.
3. The condition of the arteries and veins and of the capillary system.
4. The probable length of existence of the malady, and its likely cause.
5. The general health.
6. The secondary results of the cardiac affection.

It is on these considerations that the treatment of valvular affections turns, and the first of them is, from a practical point of view, the most important. It influences action in diverse ways. Thus, if we have a case with heart-muscle which has increased in size simply to the extent necessary to overcome the difficulty made by the valvular imperfection; in which the cavities are but little stretched; in which a stronger impulse is not associated with marked apex displacement or with greatly increased transverse percussion dulness; in which the arteries do not throb inordinately, nor the veins are turgid, nor the surface mottled and the capillary circulation sluggish; in which there is no dropsy, no respiratory embarrassment; in which the general health is good, and exercise does not produce inordinate distress—we know that compensation has followed injury, that heart-muscle and cavities are in, for that case, healthy condition, and should endeavor to keep them so by simply regulating the patient's life and habits.

No matter by what name the valvular disease is labelled, there ought to be no interference with it by drugs, certainly not by remedies which act on the heart.

But the same patient may show excessive cardiac growth and force, and be greatly benefited by cardiac sedatives; or later he presents a halting beat of the heart, the cardiac dulness has increased, so has the impulse in extent, but not in force; there is œdematous swelling around the ankles; the veins are more prominent; lungs and liver are engorged; small vessels are visible in the skin, and are tardily emptied by pressure; the stretching, faltering heart calls for support, and is rallied, made regular, and kept for a long time performing its functions admirably, by the persistent employment of moderate doses of digitalis. Yet this is the same patient; all the while the disease bears the same name. But his treatment has been followed by such striking results because the state of the heart-muscles and of the cavity has been fully recognized, and been made the basis of remedial interference.

Persons thus supported in their circulation may be kept alive for years and capable of leading useful lives. But this is not the main point I wish to bring out. It is chiefly that the same valvular disease will at different times, according to the varying state of heart-muscle and cavity, require very varying treatment. In truth, I have had cases come under my observation in which the active state of the circulation, the marked hypertrophy, the cardiac uneasiness were always greatly relieved by aconite, and so much aggravated by digitalis, which produced a sense of cerebral uneasiness and weight, that the patient had the greatest dread of this drug, even for temporary purposes; yet I have known such cases, when the heart began to weaken, owe, for years, their life to the steady use of digitalis.

The class of cases just alluded to, chronic in their course with slowly deteriorating compensation, are, usually, those in which small doses of digitalis act so favorably. The quantity required is, indeed, rarely more than ten drops of the tincture twice daily, kept up until the effect on the heart and pulse becomes perceptible—which may be in a week or in several—and then suspended, to be resumed according to circumstances. Nay, I have found a single dose of ten drops, repeated once in twenty-four hours, preferably at bedtime, show the same happy results. Some patients do better with five-drop doses, every fourth

or sixth hour. But the rather larger dose, at longer intervals, is usually the less disturbing plan. Should the digestive organs become deranged, I use digitalis by suppository; from two to four minims of the fluid extract incorporated in cocoa butter are efficient.

Digitalis acts in the cases under consideration chiefly as what is called a heart tonic; it makes the contractions of the cardiac muscle stronger and slower, it produces a fuller flow in the finer vessels. It answers, as already stated, in small or in very moderate doses. It is required in much larger amounts in those instances of valvular disease, comparatively rare, in which there is almost from the first dilatation and all the excessive feebleness of circulation this brings with it; or in which, late in the history of the valve affection, the dilatation has outstripped the hypertrophy. Under both these circumstances of cardiac weakness, digitalis may be alternated with strychnia and supplemented by alcohol.

There is yet another cardiac condition encountered in valvular disease, in which digitalis is the principal remedy, and in still larger doses. It is where the compensatory hypertrophy is gradually lessening in proportion to the valvular defect; where the venous system is becoming gorged, the breathing much oppressed, the internal organs congested; where the feet are beginning to swell; where the pulse is rapid and compressible, and the heart often fitfully excited; it is when the symptoms become rather suddenly aggravated, and a sense of weight and distress in the cardiac region suggests that the organ does not fully empty itself, that larger doses of digitalis will show a wonderful influence. Fifteen minims of the tincture every second or third hour will not only cause the struggling organ to contract powerfully and help the general circulation, but will diminish the choked condition of the cavities, notwithstanding that up to a certain point digitalis prolongs the diastole. The action of the drug is helped by ammonia, by brandy; but while given in these large doses the patient must be kept at rest. The mischief once checked, smaller doses will again show their good effects. We may meet with the condition under discussion in any case of valvular disease; undoubtedly most often in mitral complaints, but also in advanced stages of aortic regurgitation; and, if in the latter affection, we need not be deterred, on theoretical grounds, from withholding the treatment indicated.

In the remarks just made it has been assumed that we are dealing

with hearts in which the muscular fibre, however increased, is, on the whole, healthy; in other words, not in a state of degeneration. But supposing that it be; supposing that there be a granular, or a fatty, or a waxy, or a fibroid change. Is the treatment to be altered? I do not think that it can be materially modified except in the rather steadier use of stimulus; yet we will not obtain the same result from digitalis or kindred agents, and arsenic or strychnia is always worthy of trial. These are difficult cases to treat, and difficult cases to recognize. The age; the history, which shows a likelihood of fatty or other degeneration; the aspect of the patient; and the very fact that the heart-muscle does not seem to respond to cardiac tonics, give us a clew to the true character of the affection. Neither the sphygmograph nor the cardiograph helps us much in the recognition.

We have been considering heart complaints, and they are the most common, in which sooner or later the compensation is defective, and the heart has to be sustained. But there are cases, already alluded to, in which this never happens, in which no interference is required, in which the patient, if patient he can be called, has a heart quite sufficient for the ordinary purposes of life. It gives no uneasiness, and, even if aware of his cardiac malady, its existence ceases to trouble him. These are especially cases of aortic disease, narrowing or regurgitation, particularly the former, with marked, but not excessive hypertrophy. Secondary results of the cardiac affection are not seen; though, as is fully recognized, there is greater tendency to sudden death, and violent exertion must be avoided. Yet I have known persons having these aortic maladies distinguished in pursuits with constant strain; one, an officer of many campaigns; the other, a most laborious physician; the third, the captain of an athletic team at a college proud of its athletic eminence; and not one is aware of being the worse for exertion, suffering neither pain nor shortness of breath. Two have lived over twenty years since I have been cognizant of their malady; they do nothing to counteract its effect, except leading a very temperate life.

Yet another class of cases presents excessive muscular growth, and cavities that have but moderately increased. This state is more often met with in aortic affections, particularly regurgitation; but it may also happen in mitral regurgitation, with or without coexisting aortic disease. The impulse is extended, forcible, and forcible out of pro-

portion to the cardiac percussion dulness; there is often throbbing of the vessels of the neck, dull headache, tension in the pulse, and a feeling of constriction in the chest. Aconite is preëminently the remedy; it diminishes the blood pressure in the arterial system and gives great relief. I usually employ two drops of the tincture, every fourth or sixth hour, for the first few days of the treatment, and then only twice a day; or give one drop every third hour until an effect on the force of impulse and pulse is produced, and keep up this effect with a drop dose, two or three times a day, for several weeks, intermitting the treatment, and resuming it from time to time. Veratrum viride has similar applicability; it is, however, more apt to nauseate. But I have often had the happiest results from a combination of one-drop doses of aconite tincture with three of tincture of veratrum viride, and seven of tincture of ginger. It is an admirable sedative, and does not sicken.

Summing up, then, the treatment of valvular affections of the heart as they present themselves ordinarily, and basing it chiefly on the condition of the cardiac muscle and of the cavities, we find practically three groups:

Cases in which no special treatment is required.

Cases in which excessive growth and strong action call for aconite or veratrum viride.

Cases in which, either early or late, and with or without increased muscle, the heart falters and needs support, and for which digitalis, used differently according to varying indications, is the principal remedy.

This line of treatment is held to independently of the exact valve affection. It requires tact and experience to adjust it to the individual case. But when adjusted, the results are excellent.

I turn now to the other points laid down at the beginning of this paper, which are to guide our therapeutics. They will not long detain us; for they are of far less importance than the one just considered. The rhythm of the heart, its regularity or irregularity, has, indeed, been already alluded to in connection with the state of the cardiac muscle and cavities. Still there are cases, especially of mitral narrowing, in which the extreme irregularity presents a striking feature, and in which the question naturally arises, whether we cannot do something special to remedy so threatening a condition. They are

mostly cases with imperfect or weakening compensation, and, therefore, to be benefited by digitalis and remedies of that class. Yet, as an adjunct to this treatment, belladonna may be advantageously employed, and pushed to its constitutional effect. From belladonna alone I have not seen any marked results as a cardiac tonic; but, without depending entirely on it, I know it to be valuable for the relief of irregular action.

The condition of the arteries and veins and of the capillary system furnishes an indication for treatment which is apt to be overlooked. Attention is paid to the veins and to their turgescence in instances of dilated right heart and cardiac dropsy. But there is the equally important state of the arteries, of the arterioles and capillaries, and the appearance of the skin and the network of fine vessels in it, by which we can judge of the more minute circulation. Now, we must remember that the very remedy we use most in cardiac disease, digitalis, contracts the arteries and arterioles, and the indications are often to get with increased cardiac power a free flow in the vessels without resistance from them. No remedy does this; and a certain remedy of the kind is greatly needed. It is claimed that strophanthus has this valuable property, that it is a cardiac tonic which does not also contract the bloodvessels; but this is not proved—indeed, recent researches, such as those of Bahadhurji,[1] suggest the contrary. Still, the evidence is in favor of strophanthus contracting the vessels to a much less degree than digitalis. Nitroglycerine and the nitrites produce rapid and great dilatation of the vessels, but have, I think, very little effect on the muscular power of the heart. Belladonna and atropia in decided doses have somewhat the same action as nitroglycerine, less on the vessels, rather more on the heart. Why can we not learn to combine nitroglycerine or atropia with digitalis in right proportions, and obtain, where we so wish it, full cardiac power without resisting vessels?

There is, I am certain, a rich field here for accurate research.

[1] British Medical Journal, Sept. 1887; also comments on his researches by H. C. Wood (Therapeutics, seventh edition, 1888). Lauder Brunton (Pharmacology, third edition, 1888) speaks of strophanthus as not producing "so marked a contraction" of arterioles as digitalis Purdy's observations (Chicago Med Journ. and Exam., March, 1887) lead him to the conclusion that strophanthus acts only in large doses upon the vessels Zerner and A. Löw (Wien. med Wochenschr., xxx. 36-40, 1887) give many pulse-tracings and arrive at much the same results as those originally published by Fraser. They found strophanthus to cause a more energetic systole and longer diastole, without producing contraction of vessels. The whole subject needs further investigation.

While waiting for an agent which by itself has the needed qualities, we can use the remedies we possess to modify each other; and in the class of cases with sluggish capillary circulation we may also make use of gentle massage.

The probable length of existence of the malady and its likely cause must be taken into account in treatment; the former, because it gives us an idea how actively the process of compensatory hypertrophy is going on, and whether it had better be stimulated or checked. Besides, it bears on the point whether the original malady is so far off that it is still worth while treating. In this respect, then, the consideration of the duration and of the cause of the valvular lesion merges.

Now, let us consider this question of cause. It is needless to repeat all the possible causes of valvular mischief; the most prominent certainly is rheumatism, next come the degenerative changes, as of advancing years, of Bright's disease. When rheumatic, can we treat it specially? My experience says distinctly not. We possess no remedies to influence the results of the rheumatic endocarditis when the acute stage is fairly over. Indeed, if it be three months after the attack, I believe the attempt useless. Before this, it may be worth while to try a course of iodides, of blisters, of rest. It will generally fail; but I have twice seen loud murmurs, left after rheumatism, thus disappear, and, I believe, the valve restored. When the attack dates some time back, no good results come from attempts at absorption. I have several times watched the effects of long-continued, faithfully carried out trials. One, in the person of a middle-aged physician who was determined to get rid of a rheumatic mitral disease with a marked systolic apex murmur, and no signs of pulmonary congestion. He kept himself saturated with iodides for a year, only stopping for short intervals, when sickened by the drug. At the end of a year the murmur was just as distinct as before, and his general health certainly not so good; the extent of hypertrophy appeared unchanged. I watched a similar case for eleven months; the result was the same. Yet it is well in instances which have clearly a rheumatic origin, to guard against the possibility of the recurrence of the rheumatism, since this may lead to an aggravation of the valve mischief; it is well at the first sign of a rheumatic outbreak to insist on rest and to administer freely alkalies, or the salicylates. At all times, too, ought the food, the clothing, and life generally, to be regulated as it would be in any one

liable to rheumatism. The same line of thought, though not with exactly the same agents, will indicate to us how to manage the heart disease of the gouty, with the occasional appearance of large quantities of lithic acid in the urine.

With reference to atheromatous disease, with its pulse so often of higher tension than the cardiac condition would indicate, we can, with our present knowledge, do nothing for the gradual decay which is going on. Acids have been suggested, but acids will not answer. Doubtless, future therapeutics will include solvents and other means to influence degenerative states, and they will be used in cardiac affections. Viewed now, we can only say that this kind of cases requires a more constant, though varied, cardiac support than the recognizable organic mischief calls for.

There is, however, a form of valvular affection of the heart in which we can treat the malady according to its cause with the happiest results; it is the form which I have called functional valvular disorder. Since the publication of my paper on the subject,[1] I have had many more cases of it, and have learned to remedy the perverted valve action and its consequences in a number of instances which at first appeared to be incurable organic valvular disease. This was accomplished by rest, followed by graduated exercise, by careful diet, and by the persistent use of small doses of digitalis, or, in some later cases, of adonidine. The cases were chiefly mitral regurgitant affections; two of them distinctly followed heart strain from excessive rowing. They were not in any sense anæmic. In two in which the treatment was concluded within the last year, and one of which had considerable pulmonary engorgement, the valve has so completely returned to its normal action that no murmur could be detected by experts who saw them subsequently.

It seems almost needless to speak of attention to the general health, as an indication for treatment, were it not that some important considerations are involved. In the first place, it is evident that the better state we keep the blood in, the better the heart muscle will be nourished, the less likely to undergo degeneration. This is, perhaps, the reason why iron is so often thought of as a routine practice in valvular affections. Yet it is, as a rule, not a good remedy; it constipates,

[1] American Journal of the Medical Sciences, July, 1869.

produces headache, a full feeling about the heart, and is badly digested. It ought only to be given in cases clearly anæmic, or after recovery from an acute malady. Food is generally much more important than iron. It should be nutritious, easily assimilated, but never taken in large quantities at a time. Strong broths, fish, eggs, meats, poultry and game, and such green vegetables and fruit as are readily digested must form the basis of the food-supply; and those who like milk, or have no distaste for digested milk, can take either in moderate amounts to advantage. There is no objection to the use of coffee and tea if not excessive, and small quantities of alcoholic drinks are rather beneficial than otherwise in inadequate or faltering compensation. Except for gouty persons, we may hold to the axiom, that it is quite right to allow alcohol in cases to which we think digitalis applicable. The light wines are well borne and apt to be of service. Champagne is bad for most patients. I have known even a single glass produce violent palpitation, cardiac distress, and oppression. The dress should be loose-fitting and warm; and, owing to the readiness with which laryngeal and bronchial catarrhs arise, exposure to cold and damp should be avoided.

With reference to exercise, it is difficult to lay down rules. Of course, all violent exercise, like all sudden effort, is to be avoided; and in the cases with rapid circulation I believe in considerable repose. But where the heart is not acting too violently, nor too rapidly, there is no doubt that regulated muscular exercise, especially on foot, is of use, as it sustains the nutrition of the organ. It must be kept within the limits of not producing shortness of breath, and ought not to be undertaken in the face of a strong wind. Of the hill-climbing and mountain-climbing plan of treatment, recently advocated by Oertel, I have had no experience. What I have seen of the difficulty people with valvular heart affections have in living in mountainous regions, as in Colorado, does not incline me favorably to the plan. Keeping the nervous system as quiet as possible and being cheerful, are undoubtedly great aids in holding the cardiac malady at bay. Nervous people with valvular disease do badly; their excitement tells on the heart. Worry is even worse. Absence of worry means generally long life; worry, short life.

The sixth condition I laid down for the treatment of valvular disease relates to the secondary results of the cardiac affection. With

these it may be proper to consider some special heart symptoms which are at times of unusual prominence. But the attacks of palpitation and cardiac pain; the tendency to syncope; the dyspnœa; the dropsies; the affection of the kidneys; the headache and vertigo; the insomnia; the plugs that are washed into brain, or lung, or spleen, or liver; the hepatic engorgement; the catarrhal affections of stomach and upper bowel, furnish so many morbid states that it would be impossible here to consider them or their management. I will only select for discussion a few, and try to make clear some points which I have learned by experience.

As regards palpitation in cases in which it is marked, we are often met by this difficulty, that it gives a fictitious strength to the impulse. We ask ourselves, whether it would not be better to treat such cases by sedatives? Yet the pulse, though rapid, does not correspond in strength, the heart is really weak, laboring; and we shall rarely be wrong in meeting the symptoms with ammonia, with brandy, and with similar agents. Then we notice a class of cases in which palpitation is not uncommon, but in which the action of the heart is sometimes rapid, at other times slow, and is very much influenced by the least fatigue. This may happen after some illness other than cardiac, or after mental anxiety. There is a functional cardiac disorder superadded to the organic malady, which may, indeed, show fair compensation, and really be but slight. Great attention to the general health, with rest, will get rid of the added marked functional disturbance; and occasional doses of bromide added to digitalis, if this be not otherwise contraindicated, a course of cannabis indica, or of arsenic, will show good results. From opium, too, given in small amounts, we are apt to observe a happy influence.

In yet another class of cases we have a constant sense of cardiac uneasiness or actual pain as a striking symptom. In such the iodides usually do good, also wearing a plaster over the heart. It may be that plasters, as Lauder Brunton suggests, act simply by pressure; but, at all events, they act. In instances in which there is decided force to the impulse, I often order aconite plasters, of half strength; in other instances, belladonna plasters; and the relief they give makes the patients very willing to repeat them. But the best of all remedies is nitroglycerine. It is most unfortunate that this valuable agent, which lessens blood pressure and diminishes the resistance the heart

has to overcome, and which, therefore, ought to have so large a field of usefulness in valvular disease of the heart, is so repugnant to many patients, and produces headache so readily that it has to be discontinued. Yet those who can take it reap the benefit. I have refrained from quoting cases in detail, but I cannot forego citing two striking instances of its favorable use, and in one of long continuance of its administration.

Mr. A., seventy-one years of age, was obliged to retire from the management of a large business on account of shortness of breath and constant dull pain in the cardiac region. He was also much annoyed by dyspeptic symptoms. He presented a mitral incompetency with only slight compensatory hypertrophy; indeed, the impulse was not strong. He had used many remedies, and did not tolerate digitalis well on account of its disturbing the stomach. Drop doses of a one per cent. solution of nitroglycerine, increased to two drops three times a day, removed in a few weeks the cardiac pain, stopped the intermittent action of the heart, and did the dyspnœa more good than anything else. He was able to resume his occupation, reverting to the remedy as he thought he needed it.

Mrs. E., fifty-five years of age, had a terrible record with reference to disease of the heart. Her grandfather and father had both been extremely gouty, and there was reason to think had had disease of the heart. Two sisters had died of valvular disease. She herself had had swollen feet when a young woman, and other evidences of gout. But these subsided; and, as years passed by, the large joints and the muscles troubled her, and she looked upon herself as rheumatic rather than as gouty. Yet she never had an acute attack of rheumatism. She was fond of travelling, and I did not see her at times for long periods. But five years before her death I am certain that she had no cardiac malady; for, knowing the history of the family, I examined her with reference to this point. She went to Europe two years subsequently, and about that time began to notice that she could not go up hill without panting.

While at Homburg, about eighteen months before her return home, she had an attack of angina, for which nitroglycerine was ordered, with relief. A subsequent and more severe seizure six months afterward at a railroad station, after some exertion, caused her to take the remedy regularly; and she soon learned that, if she persisted in its use, she had neither attacks of angina nor the steady cardiac pain from which she suffered. She kept on with the medicine, in the shape of tablets of $\frac{1}{100}$th of a grain, twice daily, rarely oftener, for a year, stopping it only for short periods. She had mitral regurgitation with very moderate hypertrophy; tendency to pulmonary congestion and to bronchial catarrh, scanty urine with some albumin, never exceeding one-fourth of the fluid in the test-tube, sleeplessness, and swelling of hands and feet. The heart was made more regular, and the dropsy speedily

relieved by digitalis and acetate of potassium. Indeed, it was kept away by this, with the occasional substitution of caffeine. Under these remedies alone, however, the cardiac pain began to return; toward the close the weakening heart had to be sustained by the free use of brandy. For her oppression and miserable nights, dry cupping and Hoffmann's anodyne proved at first of service; she had, finally, to take full doses of morphia. The tablets of nitroglycerine were not abandoned until near the end. She died comatose.

One of the most important of the secondary results of the cardiac malady is the diminution of the quantity of the urine. Not nearly enough attention is paid to this point, and unless the urine be albuminous, it is not thought to be of any service to take its state particularly into account; yet it is very valuable to do so. Scanty urine, often of higher specific gravity than normal and full of urates, will go hand in hand with cardiac pains, with headache, and with dyspnœa. It is well known that the shortness of breath in valvular disease does not always receive an adequate explanation in the physical condition of the lungs. The explanation may be partly found in the concentrated condition of the urine, and, very likely, in some of its retained elements producing the disturbance in the capillaries of the lung or the respiratory centre. At all events, from a practical point of view, we observe that diuretics, in the condition alluded to, are most valuable in relieving the pulmonary distress and the other symptoms. Of great service is caffeine, than which, indeed, there is no better diuretic in cardiac cases, especially those with weak heart and concentrated urine, and which, also, up to a certain point has the properties of digitalis as a cardiac tonic. The dose generally sufficient is two grains of the citrate given every third hour; but it may be given in five-grain doses or more. Caffeine itself is advantageously administered, as was, I think, first suggested by Tanret, in combination with benzoate of sodium in solution. I have found a grain of each of these drugs mixed with syrup of orange flowers, or of orange-peel, and water, each half a drachm, a good formula. Some of the new salts, which are very soluble, such as the cinnamate or the phtalate, are also easily given. The former of these, as the sodio-cinnamate, contains sixty-two per cent. of caffeine, the latter fifty-six per cent., and is soluble in five parts of water. Both are adapted to hypodermic use; so is the sodio-salicylate.

Dyspeptic symptoms, due to a catarrhal condition of the stomach and bowel, are very common in valvular diseases, especially in those of the

mitral and tricuspid valves. They may or may not be associated with an engorged, torpid liver; they may or may not be in the form of painful digestion. In either case the failing appetite is apt to be treated by tonics, often by iron. These are not, in my judgment, the right remedies. I believe purgatives are; they strike directly at the morbid state, and subsequently some bitter, or small doses of nux vomica, will restore the desire for food. Purgatives are not given as often as they ought to be in valvular disease of the heart. There is the fear of weakening the patient; which they do not, if not abused. They not only remedy the stagnation in the portal circle and remove the catarrhal condition, but they lessen the liability to dropsy. The old treatment of an occasional mercurial was good treatment; and calomel may be beneficial in other ways in affections of the heart, especially those with dropsical tendencies, than through its diuretic action, which is now receiving so much attention.

It is impossible in examining the treatment of diseases of the heart, whether of the disease itself or of its consequences, not to be struck with the important part digitalis plays. And the question naturally arises, whether any of the newer remedies can take its place. I have tried them all, and I believe there is not one which is as trustworthy, as valuable; not one is at the same time so good a cardiac tonic and a diuretic. But undoubtedly digitalis cannot be kept up uninterruptedly, and it is apt to produce, after a time, derangement of the digestive organs. Some cannot take it at all; and as in any form of tonic, so with this cardiac tonic, we get better results by occasional change. I hold caffeine, strophanthus, and adonidine to be the best substitutes. I have already spoken of caffeine. From adonidine I have witnessed, in one-tenth to one-fifth of a grain doses three times a day, some admirable results; but more in cases of functional than of valvular disease of the heart. Yet even here I have known it to act as an excellent heart regulator. So does strophanthus, which I have, moreover, often seen strikingly influence irregularity and dyspnœa. Its action is very rapid, but not so permanent as that of digitalis, and though much is claimed for it as a diuretic, its influence in this respect is inferior both to digitalis and to caffeine. It would seem specially applicable to cases with defective power and high arterial tension, as sometimes met with in the heart lesions of Bright's disease; but from actual experience I am not yet certain on this point. Convallaria has, on the whole, disap-

pointed me in the treatment of valvular disease, though I think it is of value in palpitation of the heart and in other forms of functional disorder. Cocaine answers very well in some cases; it is certainly both a cardiac stimulant and tonic and not devoid of diuretic powers.

A remedy which I am using now a great deal is chloride of barium. Since I became acquainted, through the investigations of Boehm and of Bartholow, with its physiological action and learned that in this it resembles digitalis, I have prescribed it repeatedly in valvular affections. I find it both a general tonic and a cardiac tonic, a remedy that increases the tone in the bloodvessels, a fairly good diuretic, and one that can be taken for a long time without disordering the stomach. It may be even, as Kobert[1] shows, administered hypodermically. The dose in which I have given it by the mouth is one-tenth of a grain in pill, three or four times daily; a rather larger dose is, however, admissible. In very decided amounts it is apt to produce diarrhœa. As Bartholow points out, it has many incompatibles, and it is best not to give it in combination. Among its properties I have noted that it lessens cardiac pain. I learned this from the case of an elderly gentleman with a mitral lesion, regurgitation with some narrowing and defective compensation, in whose case pain or constant cardiac uneasiness was a prominent feature. Digitalis did him in this respect no good and was losing its effect in steadying the irregular heart. Chloride of barium in one-tenth of a grain doses improved him greatly; the oppression was relieved, the heart became more regular, the cardiac distress disappeared. He has been more than once benefited for a long time by a three weeks' course of the remedy.

I must not bring this paper to a conclusion without mentioning a point of which I know the great value,—to make periodical examination of persons affected with valvular disease. I am not speaking of those in whom serious symptoms call for constant supervision; rather of those who, under our advice, take little or no medicine. In them, too, it is true that the heart of to-day may not be the heart of a month hence. Yet they are the ones chiefly in whom beginning changes can be most readily met, and whose lives, with the aid of treatment when necessary, can be greatly prolonged. Let them be made aware of the importance of skilled supervision. It will not mean needless interfer-

[1] Therapeutic Gazette, June, 1887.

ence; it will mean judgment as to when interference is really helpful. In valvular disease, as in other instances of disease of the heart, advance in knowledge is demonstrating how the arrow in the side can be kept from being fatal.

DISCUSSION.

DR. F. C. SHATTUCK: I would like to ask Dr. Da Costa whether his experience with strophanthus has led him to share the view expressed by Dr. Fraser with regard to its action—that while the drug is an efficient promoter of the vigor of the cardiac contractions, it does not, as does digitalis, at the same time increase the resistance in the peripheral circulation by contracting the bloodvessels.

DR. S. C. CHEW: In my experience with strophanthus it has varied with different preparations of the drug. The tincture which I first used was obtained from Professor Fraser, of Edinburgh, and gave most satisfactory results. Other preparations used since then have in some instances proved very effective, and in others have disappointed me. Whether strophanthus can add to the contractile power of the heart without increasing arterial resistance, as claimed for it, is open to doubt.

DR. DA COSTA: Judging of strophanthus clinically I have not been able to satisfy myself that it has absolutely the power claimed for it, of acting on the heart without contracting the arteries.

DISTURBANCES OF THE HEART RHYTHM WITH REFERENCE TO THEIR CAUSATION AND THEIR VALUE IN DIAGNOSIS.

By G. BAUMGARTEN, M.D.,
OF ST. LOUIS.

INTERMISSION and irregularity of the pulse never fail to attract the attention of the physician, and have been the subject of much discussion. Nevertheless, they are not commonly utilized as a diagnostic resource in a degree at all commensurate with the possibilities; and this partly for the reason that we have not become accustomed to trace their meaning systematically, as we should trace that of a cardiac murmur, of an albuminuria, or any other physical sign; nor do our text-books afford us much help in distinguishing cases in which the character of the heart's rhythm may have a deciding influence in questions of diagnosis or prognosis. In a given case, the mere fact of irregularity, by itself, may have little or no import; yet when attention is given to the character of the irregularity and to its conjunction with certain other conditions of the circulation, it may acquire diagnostic significance of great value. A systematic study and disposition of certain facts, facts well known to you, may possibly secure for us a point of view from which greater precision of interpretation in the individual case becomes possible. If the desire for brevity should betray me into unduly dogmatic language, I trust you will supply the necessary grain of salt.

Above all things is it desirable to group the alterations of rhythm according to their fundamental forms, a demand which the majority of text-books do not even attempt to fulfil. Anomalies of rhythm of the arterial pulse may be conveniently classed under three heads:

I. Mere fluctuations in the frequency of the heart-beat from hour to hour, from minute to minute, or even in half-minutes; the uneven,

unsteady pulse, slowed, as a rule, but easily impressed to greater frequency by slight causes, the so-called *cephalic pulse*, as seen in meningitis and other cerebral disturbances. The most common mode of origin of this pulse is in compression of the brain, which, as it arises, first slows the pulse, then makes it irregular and very frequent. The slowing is attributable to irritation of the pneumogastric centre, the increased frequency to paralysis of the same; *i. e.*, moderate pressure excites, stronger pressure paralyzes it; the irritability of the centre is increased before it is lost. The greatest irregularity (unevenness) of pulse is seen at the time when the centre begins to lose its irritability. While retarded, the pulse is large and full; when frequent, it becomes small and vanishing. During the transition, the pulse-rate is subject to sudden change; the slowed pulse of 60 rises in a few minutes to 100, to 120, to 160, and in a short time again subsides. But, for the time being, the contractions of the heart are not really irregular, though often they are of unequal force.

This form of pulse invariably points to cerebral trouble—compression of the brain from some cause, as from meningitis, from meningeal or cerebral hemorrhage, etc.; local pressure in the region of the medulla oblongata; concussion of the brain, etc.

A similar pulse is sometimes encountered *sub finem vitæ*, as the expression of failing of the vagus centre, either from the asphyctic condition of the brain, or from œdema.

II. Variations in frequency as well as in amplitude of pulse consonant with the respiratory movements, in such manner that the pulse-beats corresponding to inspiration are more frequent and less ample than those which coincide with expiration. This disturbance of rhythm may be carried so far that the inspiratory pulse-waves fail to reach the wrist, or at least to become imperceptible to the finger. Of this form of altered rhythm there are two varieties: (1) that which depends on mechanical compression of the aorta during inspiration, the original *pulsus paradoxus* of Kussmaul, which by him was considered pathognomonic of fibrous mediastino-pericarditis, but which has also been observed in some cases of copious effusion into the pericardium. This variety evidently has nothing to do with the rhythm of the heart, nor, directly at least, with the frequency of its contractions. (2) The very similar pulse resulting from exaggeration of the normal influence of the respiration upon the heart's contractions.

This second form has been observed in stenosis of the air-passages and in capillary bronchitis, *i. e.*, under conditions which exaggerate the respiratory movements themselves.

The pulsus paradoxus of the first variety may be recognized as a sign of fibrous mediastino-pericarditis by the swelling of the jugular veins during each inspiration; the fibrous bands which, when tightened by the inspiratory forward movement of the sternum, press upon the aorta, also compress the superior vena cava at the same time;—as a sign of extensive hydro-pericardium by the permanent swelling of the jugulars. But in the absence of these symptoms on the part of the jugular veins, the pulsus paradoxus is to be interpreted as caused by exaggerated respiratory movements, as has been observed in croup, in paralysis of the crico-arytenoid muscles, in compression of the trachea, and in capillary bronchitis.

With a moderate degree of this form of paradoxical pulse I have become familiar as a harmless phenomenon in sleeping children. It is very likely the pulse to which Stoll refers in the sentence quoted by Nothnagel:[1] "*In pueris dormientibus*," etc. "In sleeping children, since in them the intermittency is almost a regular thing, it is not to be considered so dangerous." It is not really an intermittent pulse, however, but, as I can aver from numerous positive observations, a pulsus paradoxus, probably due to the full respiration, whose influence on the innervation of the heart predominates in the deep sleep of the child (more than in the adult) over the other regulating influences on the part of the central nervous system.

III. The third group, finally, comprises all the numerous changes in rhythm more commonly seen in the form of intermittence, of alternating and bigeminous rhythm, and of complete irregularity. They are closely allied among themselves, and result from varied combination of two factors, viz.: (1) the feebleness or suppression of a cardiac systole recurring at longer or shorter intervals with more or less regularity; and (2) variations in the length of the individual pulses, which may also occur regularly or with entire irregularity. The types of pulse which result are mainly these:

[1] Deutsches Archiv f. klin. Med., xvii. p. 190. The entire paragraph reads: "*Pulsus intermittens in una pulsatione pessimus in acutis, et inflammatoriis febribus, præsertim si alia vigeant gravia accidentia. Non ita malus, si morbis pectoris superveniat, in quibus familiaris est, aut in morbis senum, aut in plethoricis, aut in pueris dormientibus, in quibus omnibus intermittentia fere cum familiaris sit, non ita perniciosa æstimanda.*" Max Stoll, Ratio medendi in nosocomio practico Vindobonensi. Viennæ Austriæ, 1790. Pars vi. Sect. i, § 67.

a. The *intermittent* and the *deficient* pulse: at uncertain intervals one beat of the heart is feeble or absent.[1]

b. The *pulsus alternans:* full and feeble systoles (high and low pulse-waves) alternate regularly.

c. The *bigeminous pulse:* a full systole is followed, after a brief diastole, by a short and incomplete systole, and this in turn by a long diastole;—if, in such a rhythm, the feeble systole is insufficient to start any pulse-wave at all, there results the phenomenon commonly called intermittent hemisystole of the heart, or more properly *heart-bigeminism*, when two systoles are represented by only one pulse at the wrist; and

d. The wholly *irregular pulse, arhythmia or delirium cordis*, produced by both factors combined without rule, both the force of the heart's contractions and the time of their recurrence varying almost at every step.

The four types enumerated are subject to many modifications; among them I will only mention that the alternating and the bigeminous pulse, of a sort of trochæic rhythm, are often converted, by repetition of the feeble systole, into a dactylic meter, when the bigeminous pulse becomes trigeminous (and similarly even quadrigeminous, etc.). These details of rhythm can be satisfactorily recognized only with the aid of the sphygmograph. It is a mistake to think these *allorhythmic* pulses (*b* and *c*) uncommon; in my experience they are of frequent occurrence.

In addition to the four types named, it appears to me that the extremely slow pulse (*pulsus rarissimus*), even if beating at regular intervals, is to be counted among the disturbances of rhythm; in its etiology, in the circumstances under which it is encountered, in its clinical meaning, it has more features in common with arhythmia, than with a mere change of pulse-rate. Not to speak of the pulse in heart-bigeminism, which is, so to speak, a bigeminous pulse with the feeble twinbeat left out,—I have observed cases in which a regular pulse, say of 30, in a short time became a regular intermitting pulse of 60, in which two full beats were followed by a complete intermission; it is difficult not to infer that in the pulse of 30, each beat was followed

[1] The pulse has been called deficient when the absence of heart sounds proves the actual omission of a systole; intermittent when, as is more common, the pulse-wave started by the systole is too feeble to reach the wrist.

by two intermissions. Lately, within my observation, a young man dying of diphtheritic paralysis of the heart, began by having a frequent and irregular pulse, in which gradually more and more of the feeble beats dropped out, lowering the actual number of palpable pulsations to 67, to 60, to 52, to 36, the intervals becoming more and more regular, until he ended with a very regular pulse of 25 per minute. The very rare pulse of atheroma of the coronary arteries —I have seen it as low as 12 per minute—never *quite* regular, is undoubtedly of this character.

I hold that all types of this group are related to each other not only in form but in origin. Gradual transitions from one to the other are often met with. Nearly all causes of the one may produce the other. They are mainly indicative of *degrees* of disturbance, in such wise that the intenser action of the cause produces the more complete arhythmia. And if this is true, then it need create no surprise that different forms of irregularity occur under apparently similar clinical conditions; nor is there any warrant for separating even the intermittent pulse from the wholly irregular pulse, so frequent are the transitions between these forms in the same case under only slightly altered conditions, as of posture, of labor and rest, of emotion and mental repose, of cold and heat, before and after food. These transitions nowhere prove themselves so distinctly as the results of *degrees* of noxious influence as in the cases of intoxication by tobacco, coffee, tea, etc. Nevertheless, in judging of the semeiological value of arhythmic pulses, sufficient account is not taken of the particular forms of irregularity. The usual statement of the text-books that the pulse is irregular must be deemed incomplete.

Moreover, as a symptom of diagnostic or prognostic import, irregularity of pulse acquires its real value only when taken in connection with its frequency and with the degree of arterial tension. An irregular small and soft pulse has a very different meaning from a slow and incompressible pulse showing the same irregularity.

Sommerbrodt held that all forms of allorhythmia and arhythmia are dependent upon disturbed innervation—*i. e.*, I suppose, upon altered function or structure of either the musculo-motor ganglia of the heart, or the fibres of the vagus or sympathetic, or their cerebral centres. I doubt if this be not too definite a statement in that it excludes the muscular fibres of the heart. Clinically, at least, we cannot divide up

the nerve-muscle apparatus of the heart, so as to decide, in a given case, whether the muscular fibres, the intracardial ganglia, or the connecting nerve-fibres are at fault. And if we cannot, then it is impossible also to prove (or to disprove) that the casual intermission of a beat, for instance, is due to inhibition (*i. e.*, a phenomenon of irritation) or to failure of either nerve, ganglion, or muscle to respond to the normal stimulus (*i. e.*, a phenomenon of depression). With our present knowledge, implicit adherence to Sommerbrodt's statement can only hamper the clinical estimation of the irregular pulse. On the other hand, much present difficulty of clinical interpretation can be avoided by considering the nerve-muscle apparatus as a unit, without attempting to prejudge the question which constituent of it is mainly or primarily affected.

In trying to classify the irregularities of pulse according to the clinical conditions that give rise to them, I shall first dispose of the rarer and less important causes, premising only, that a disproportion between the strength of the heart and the demands made upon it is the fundamental condition of irregularity in most cases, and that, therefore, all other causes are the more potent, the weaker the heart is.

1. One large group of the arhythmias is due to affections of the cardiac centre in the medulla oblongata. Both irritation and depression of the cardiac centre seem capable of disturbing the rhythm of the heart; several conspicuous instances of pulsus alternans and of pulsus bigeminus have come under my observation in cases of cerebral hemorrhage and of softening. Concussion of the brain sometimes causes intermittent and allorhythmic pulses. The more pronounced arhythmia said to have been seen in syncope seems referable to depression of the cardiac centre from cerebral anæmia. Psychical influences are known to be causes of temporary arhythmia, most frequently in the form of intermittence. Whether disease of the trunk of the pneumogastric is ever capable, alone, of causing changes of rhythm is extremely doubtful. Nor is there any clear proof that diseases of the sympathetic fibres, apart from lesions of their centre, affect the rhythmic beat.

2 The nerve apparatus of the heart, both cerebral and intracardial, is, however, often disturbed by reflex influences starting from distant organs, as in dyspepsia, in hepatic affections, in some respiratory lesions, and notably in diseases of the kidneys. Apart from the

lesions of circulation secondary to or concomitant with renal affections, we not seldom witness in acute nephritis an irritability of the heart, difficult to explain except by reflex, which shows itself first in great frequency and smallness, and ultimately in complete irregularity of the pulse; in other cases a pulsus alternans persists for long periods —weeks.

The arhythmia sometimes seen in such neuroses as pure angina pectoris, chorea, etc., will usually fall under this head. To say that some cases of irregularity of the heart are due to an essential neurosis of the cardiac nerves and ganglia is, of course, begging the question. Of late, distinct organic lesions of the intra-cardiac ganglia have been observed, and upon such these cases may be based; but our knowledge in this direction is fragmentary.

3. A third group of arhythmias is that caused by the action of drugs upon the nerve-muscle apparatus of the heart. The action of the every-day heart poisons, tobacco, coffee, and tea, is well known; a conspicuous instance of a very persistent trigeminous pulse from chronic nicotism is fresh in my memory; but simple intermittence and complete arhythmia are quite as common, indicating either different degrees of intoxication, or different degrees of integrity of the heart itself. In a case of delirium tremens (if you will permit the expression "for short") from abuse of tea and abstinence from food, I have been able to observe almost all forms of rhythmic aberrations in quick and endless change. Digitalis, belladonna, aconite, calabar, and similar poisons induce arhythmia under certain circumstances. A number of other drugs accused of the same action, such as chloroform, tartar emetic, arsenic, seem to produce the irregularity indirectly. Thus, Petrina[1] found that pilocarpin injected under the skin induced an allorhythmia (p. bigeminous) or arhythmia, after every injection, in some individuals "in whom there was reason for considering the muscular or the valvular apparatus of the heart as not quite normal."

4. For, after all, the majority of cases of disturbed heart-rhythm are owing to changes in the heart itself. The condition common to these cases is incompetency of the heart in the broadest sense,—a disproportion between its motor power and the work to be done. If

[1] Deutsches Archiv f. klin. Med., xxi. pp. 416 et seq., 1878.

observation of its rhythm can aid in determining the condition of the heart as a motor organ, the value of this class of symptoms must be apparent, the more so when the physician realizes that a correct estimate of the vital power of a diseased heart is of even greater importance to him than a minute anatomical diagnosis. Where the irregularity has its origin in the heart itself, the power of its muscle-nerve-apparatus is not, or is no longer, sufficient for its task. This principle is important in deciding the question whether the cause of the arhythmia is situated in the heart. The arhythmia of the incompetent heart can be temporarily increased by increasing the demands on the heart; exercise increases the irregularity. More suggestive still is the fact that a moderate degree of allorhythmia which is observed in the erect or sitting posture, often ceases when the patient lies down,—an observation which, within my experience, justifies the diagnosis of its cardiac origin.[1]

a. Milder grades of incompetency of the heart result from defective blood supply and malnutrition of the cardiac wall, from long-continued fevers, and from dilatation of the heart. We may expect moderate degrees of irregularity in anæmic and cachectic patients; their small, soft, and usually frequent pulse may remain regular until some additional demand, as by exercise, by some sudden new stimulus, by a sudden rise in blood pressure, renders it intermittent or bigeminous.

The same not seldom happens toward the end of infectious fevers. Landouzy and Siredey[2] observed in all fatal cases of typhoid fever a myocarditis with atrophy or fatty change of the muscular fibres; above all, in those cases which terminated by collapse, and "in which the symptoms during life had been irregularity of rhythm of the pulse and rapid failure of the heart." In pneumonia, also, failure of the heart makes itself known by unequal filling of the artery, then intermissions, and at last perhaps entire irregularity of pulse.

Dilatation from sudden severe strain or from habitual overwork, as in the case of military heart-strain, is very sure to produce, at first, a small, soft, accelerated pulse, and later an intermittent or even wholly irregular pulse of great frequency and low tension. When

[1] I have also known the reverse to take place—a bigeminous pulse to disappear whenever the patient rose to his feet—in a case clearly not of cardiac, but of nervous origin.

[2] Revue de Méd., 1887; quoted in Practitioner, January, 1888, p. 64.

dilatation is the attendant of valvular disease, its beginnings are usually compensated by hypertrophy, and the pulse remains regular; but with beginning failure of the heart, the pulse passes more or less gradually through the consecutive degrees of rhythmic disturbance. Even in the best compensated heart the rhythm is affected temporarily by those causes which will produce arhythmia in hearts weakened through other than valvular affections—by unwonted exercise, mental distress, intercurrent disease, malnutrition. The irregularities of pulse arise sooner, as a rule, in mitral than in aortic disease, sooner in insufficiency of the valves than in stenosis of the orifices. When the limit of compensation has been reached the disturbance of rhythm becomes permanent, and often attains to the highest degree; the cardiac muscle has become permanently incompetent, and undergoes serious nutritive and structural changes. Hence, when in the course of a valvular defect the pulse becomes persistently irregular, it signifies that the hypertrophied heart is now falling a prey to those changes which set a bar to all further compensatory growth; and the greater the previous hypertrophy, the more complete the subsequent arhythmia—a rule which holds true in enlargement of whatever origin.

b. By far the most frequent cause of complete arhythmia is that group of organic changes usually classed as myocarditis. This field, upon which I have already trenched, is far too large to harvest in the intended space of this paper; I must confine myself to the more common instances. The structural lesions named chronic myocarditis, when they have reached an extent which enables us to diagnosticate them, always make the pulse small, frequent, and completely irregular, the irregularity being persistent, not paroxysmal; in the absence of this persistent arhythmia, indeed, the diagnosis is commonly impossible. No doubt, patients die before this stage has been reached, who had a regular pulse and yet show structural alterations of the myocardium *post mortem;* but such cases are practically beyond positive diagnosis during life.

Acute myocarditis is also characterized by an arhythmic pulse, very small and often extremely frequent; and its irregularity is an essential factor in the diagnosis.

c. The severer lesions of circulation in the substance of the heart are likewise prolific causes of arhythmia; above all, sclerosis and narrowing of the coronary arteries, with its almost certain result of fatty

degeneration. The pulse is usually slow, also—sometimes extremely so. While some authors, like Leyden, make small account of irregularity of the pulse in sclerosis of the coronary arteries, I do not remember a single case of it in which the pulse was not either uniformly allorhythmic, or, by spells, completely arhythmic; and the latter condition I consider much the more common. Extreme slowness of pulse, brachycardia, which is closely allied to the arhythmias, is also a common event.

The use of the irregular pulse in diagnosis is, of course, greatly assisted by proper attention to its other qualities. The strength of the pulse, its frequency, the persistency of the irregularity, should be thought of in immediate connection with the rhythm itself.

Thus (1) as to compressibility of the pulse: Those arhythmias which depend upon incompetence of the heart occur in *soft* pulses. With competent, especially hypertrophied hearts, the irregular pulse may be of *normal* resistance (as in arhythmias from nervous and reflex causes), or even *hard* (as in vaso-renal affections and in early stages of atheromatous disease).

(2) As to frequency of the pulse: The irregular *slow* pulse (excepting the cephalic pulse, due to intra-cranial causes) is probably indicative of fatty heart the result of disease of the coronary arteries. Fatty degeneration of the heart, by itself, does not make the pulse irregular;[1] but when a temporary over-exertion causes such a heart to stumble, the pulse is not only irregular, but also *frequent*, and small, and soft; when the fatty change is the sequel of atheroma of the coronary arteries, the irregular pulse more likely is retarded (but the blood pressure is low, even though the radial artery feels rigid under the finger). Mere weakness of the heart from any cause, when it makes the pulse irregular, also makes it *frequent*.

(3) As to the persistency of the irregular pulse: An arhythmia which is permanent, never disappearing entirely, points to organic change in the heart itself; one which occurs only when the heart is temporarily subjected to more than ordinary work, indicates the rela-

[1] Many obese persons with fat-encumbered hearts have a slow pulse; whether the store of fat in and about the heart's muscle is the sole and direct cause of the intermittence seems doubtful to me, since I have seldom observed this intermittence in *young* persons of that description. Possibly, the pressure exerted by the exuberant fat has, in time, an effect upon the myocardium similar to that of calcified coronary arteries.

tive incompetency of the heart; when it occurs only in paroxysms, and with absolutely free intervals, it points rather to lesions of the cardiac nerve-apparatus, organic or functional.

Finally it should be said, that in aged people the heart's rhythm is more easily disturbed than in the young; that the irregular pulse is more frequently encountered and of less serious import, on the whole, in advanced age than in youth and early manhood; but that the irregular pulse in very young children is also "*fallicissima res.*"

DISCUSSION.

DR. E. T. BRUEN: I desire to refer to an article in the *Transactions of the Pathological Society of London*, by Dr. C. Hilton Fagge, in which a series of cases of fibroid disease of the heart are recorded, and in which a markedly slow pulse was a very characteristic feature. I think it an important matter to recognize that a slow pulse is so common in fibroid disease, since the physical signs of this affection differ materially from those of fatty degeneration of the heart. I have seen one case in which the pulse ranged from 18 to 22 pulsations per minute for a period of three months—when death occurred and pronounced fibroid changes in the heart were found.

DR. R. T. EDES: I wish to call attention to one thing I have noticed, and that is, the occasional intermission in certain cases of pericarditis in which a beat or two falls out or becomes much weaker during the inspiration.

DR. WILLIAM OSLER: There are one or two points to which I wish to refer: certain individuals suffer with extreme irregularity of the heart for several years, without its producing the slightest inconvenience. In a foot-note to my article in Pepper's *System of Medicine*, I mention the case of a man of fourscore who had extreme irregularity of the heart since his fortieth year. Another point to which I would call attention is the extreme irregularity which may exist in certain cases of mitral disease with apparently complete compensation, an irregularity often obstinate to every kind of treatment.

DR. H. M. LYMAN: In cases in which there is no valvular lesion, no dilatation of the heart; in which, in fact, the phenomena are merely functional, I have noticed disturbances of the heart-rhythm. Many of them seemed to be entirely due to a disturbance of the excretory organs of the body. I have found them manifest themselves in persons of a nervous temperament, overworked, and harassed by business, but presenting no lesions of the kidney or heart, or organic changes of the liver; and they were often associated with

some degree of gastro-intestinal catarrh. Such cases as that I have found not appreciably benefited by digitalis, or other heart tonics, but greatly relieved by measures which tend to stimulate the action of the liver and kidneys. They frequently manifest symptoms of muscular rheumatism and lithæmia. Cases of this kind are very much relieved by laxatives associated with hepatic stimulants, blue pill with a little colocynth. These often give more relief than any specific cardiac drug.

DR. A. E. SANSOM, of London: Great irregularity of the heart's action is very often consistent with perfect health. I have had several striking illustrations of this. I think it the bounden duty of the physician, unless this irregularity is associated with cardiac changes, or unless we feel there is dilatation or degeneration, to try to reassure the patient, and direct his thoughts away from the heart.

DR. W. H. DRAPER: Some years ago my attention was called, by an article by Dr. Richardson, of London, to a class of cases which have been alluded to in this discussion, in which irregularity of the heart's action persisted for many years. Dr. Richardson traced many of these cases to extreme nervous shocks, loss of property, grief from the loss of a relative, etc. I have been able to confirm Dr, Richardson's observation in a number of instances. I would like to ask Dr. Baumgarten if that agrees with his observation.

DR. BAUMGARTEN: Perfect consistency of good health with irregular pulse has been mentioned. The intention of my paper was to try to arrive at a systematic view of altered rhythms. Of course, it is not my object to lay stress on the exception rather than the rule. One who has observed disturbed rhythm with perfect health has certainly observed a great many cases of arhythmia with impaired health. A number of these cases of irregular action of the heart in "healthy" persons may be traced to the influence of tobacco, coffee, alcohol, etc. Psychical influences have been enumerated as causes of irregular pulse in my paper.

THE PULSE-WAVE VELOCITY AND VENTRICULAR CLOSE-TIME IN HEALTH.

By JAMES K. THACHER, M.D.,

PROFESSOR OF PHYSIOLOGY AND CLINICAL MEDICINE IN YALE UNIVERSITY, NEW HAVEN, CONN.

SINCE Henderson[1] in 1837 first claimed that the interval between the apex-beat and the pulse in the arteries was greatly prolonged in aortic regurgitation, this interval has attracted considerable attention from clinical observers, but a definite conception of its relations to the various morbid conditions of the heart and arteries has not yet been established. A brief account of opinions expressed in confirmation and negation of Henderson's view, and of such other claims as may have been made for the diagnostic importance of this interval in other directions, will, I think, suffice to show the existing need of a thorough working-over of the whole subject.

The doctrine at first met with considerable opposition. Aran[2] (1842), Requin[3] (1846), Forget[4] (1851), Grisolle[5] (1852), denied Henderson's claim and held that the interval, if ever prolonged in cases of aortic insufficiency, never exceeded what was incident to the enfeebled circulation. Following this period of opposition came one of comparative prosperity for the doctrine.

Bamberger[6] (1857), in accord with Henderson, recognized an increased interval as a peculiar sign of aortic regurgitation, and stated that it was present even when there was extensive arterio-sclerosis: so also Oppolzer[6] and Quincke.[6] Flint[7] (1859) sustained the doctrine for

[1] Henderson: Edinburgh Med. and Surg. Journ., 1837, vol. ii. p. 364. On inadequacy of the valves of the aorta, see also London and Edinburgh Monthly Journ., 1843, vol. iii. pp. 451, 452.

[2] Aran: Recherches sur les signes et le diagnostic de l'insuffisance des valvules de l'aorte, Arch. Gén. de Méd., 1842, tome xv. p. 265.

[3] Requin: Traité de pathologie médicale, tome ii. p. 783, 1846.

[4] Forget: Precis theorique et pratique des maladies du Cœur, des vaisseaux et du Sang., p. 184, 1851.

[5] Grisolle: Traité de pathologie interne, cinquieme ed., tome ii. p. 269, 1852.

[6] Teste Grunmach: Virch Arch., Bd. 102.

[7] Flint: Diseases of the Heart, 1859, p. 141.

certain cases. Friedreich[1] (1861), however, denies that he is able to confirm it. Duchek[2] (1862) pronounced in its favor, but said that the same symptom was present in typhoid, in severe pneumonia, and elsewhere; Roncati[3] (1868) recognized the prolongation of the interval only when the disease was very much advanced.

Traube[4] (1869) accepted Henderson's doctrine, but found that the prolongation of the interval was not present when the arteries were extensively sclerosed. Walshe[5] (1873) admits the doctrine for certain cases but denies it for others. Tripier[6] (1877), in a somewhat elaborate examination of the subject, finds that the cardio-carotid interval is prolonged in these cases except where there is extensive arteriosclerosis.

This seems to close the pregraphic period of the discussion, and this period seems to fall into two nearly equal parts, in the first of which Henderson's doctrine was denied, while in the second it was with a fair, but not absolute, unanimity sustained. In the next year Bozzolo and Fiori[7] (1878), working by the graphic method, were unable to confirm Tripier's results. Francois-Franck[8] (1878), working also by the graphic method, came to the conclusion that while there is an apparent increase in the cardio-arterial intervals (due to the auricular shock at the apex being taken for the ventricular shock), the real interval is shortened. Keyt[9] (1879) also, independently of Francois-Franck, came to the same conclusion. Grunmach[10] (1885) recognizes an abnormal delay in cases of aortic regurgitation with inadequate compensation, and refers its occurrence in this and in other cases of cardiac and general disease to a mere lowering of the blood-pressure.

[1] Friedreich: Handbuch der Herzkrankheiten, 1861. Virchow's Hdbk., Bd v. Abth. 2.

[2] Duchek: Die Krankheiten des Herzens, etc,. 1862.

[3] Roncati: Indirizzo alla diagnosi delle malatie del petto, del ventre e del systema nervoso. Napoli, 1868, p. 269.

[4] Traube: Gesammelte Beiträge zur Pathologie u. Physiologie, 1869, Bd. iii. S. 168.

[5] Walshe: Practical Treatise on Diseases of the Heart and Great Vessels, 4th Eng. ed., 1873, p. 70.

[6] Tripier: Revue Mensuelle de Méd. et de Chirurg., Janvier, 1877. Du retard de la pulsation carotidienne sur la systole cardiaque dans l'insuffisance aortique.

[7] Bozzoli and Fiori: Intorno al significato diagnostico del ritardo del polso della carotide sull'impulso del cuore. Torino, 1878. Gior. d. r. Acad. di med. Torino, 1878, 3 s. xxiii. 161-178, 1 pl.

[8] François-Franck: Comptes Rendus des Séances et Memoires de la Société de Biologie, Mar. 16, 1878. Du retard reel et du retard apparent dans l'insuffisance aortique et dans l'aneurysme de l'aorte avec ou sans insuffisance aortique; du retentissement de la systole d l'oreillette gauche jusque dans la carotide chez les malades atteints d'insuffisance aortique.

[9] Keyt: Cincinnati Lancet and Clinic, March 22, 1879. See, also, Boston Med. and Surg. Journ., Sept. 30, 1880, and collected papers entitled Sphygmography and Cardiography, 1887.

[10] Grunmach: Ueber die Pulsgeschwindigkeit bei Erkrankungen des Circulationsapparates sowie bei Einwirkung toxischer Mittel, Virch. Arch., Bd. 102, 1885.

All of these observers used the graphic method, which thus seems to have failed to bring that definite decision of these questions which might have been expected from it.

But while these opposing opinions formed the outcome of clinical observation on what seemed a simple matter of fact, the physiologists had also devoted considerable attention to these cardio-arterial intervals. Marey[1] in 1863 recognized the existence of an interval between the beginning of ventricular systole and the opening of the semilunar valves, and estimated it as about 0.1 sec. This time, during which the ventricular is rising to equal the aortic pressure, was independently discovered by Czermak[2] in the following year and determined as amounting to upward of 0.06 sec.

In 1874, A. H. Garrod[3] proposed for this interval the name of *systasis*, while Keyt in 1879 suggested that of *presphygmic interval*, Martius[4] (1887) that of *close-time* (Verschlusszeit), and Edgren[5] (1887) that of the *latent period of the pulse-curve* (Pulscurvans latenstid). The fact that the total cardio-arterial interval is composed of two parts, the ventricular close-time and the time of arterial transit, came to be recognized by clinical observers, but Keyt made it especially prominent and referred the variations of the cardio-arterial intervals in valvular disease to variations in the close-time.

Grunmach, while he recognized the close-time, did not admit its variability, but, rather curiously, simply subtracted 0.07 sec. (the estimated value of the close-time) from his cardio-arterial intervals to obtain the aortic-arterial intervals as data for calculations of velocities.

The other valvular defects have attracted less attention in their relation to cardio-arterial intervals. Bamberger[6] (1857) and v. Dusch[7] (1868) claimed that there was increase of these intervals in at least some cases of aortic stenosis; so, too, Guttmann[8] (1886) and,

[1] Marey : Physiol. med. de la circulation du Sang. Paris, 1863, pp. 190, 191.

[2] Czermak : Mittheilungen aus dem physiologischen Privatlaboratorium in Prag, 1864.

[3] A. H. Garrod : On some points connected with the circulation of the blood arrived at from a study of the sphygmographic trace. Proc. Roy. Soc., xxii. pp. 291–293.

[4] Martius : Graphische Untersuchungen über die Herzbewegung. Zeitschr. f. klin. Med., Bd. xiii. p. 327, ff.

[5] Edgren : Kardiografiska och sfygmografiska studier. Nordiskt Medicinskt Arkiv, xix. No. 19, II., 1887.

[6] Bamberger: Lehrbuch der Krankheiten des Herzens, p. 284, 1857.

[7] Theod. v. Dusch : Lehrbuch der Herzkrankheiten, p. 227, 1868.

[8] Guttmann : Lehrb. d. klin. Untersuchungsmethoden, 6te Aufl., 1886, p. 271.

in uncompensated cases, Grunmach.[1] Keyt[2] denies that the close-time is affected in this condition.

In mitral insufficiency Keyt holds that the close-time is very much exaggerated and that it furnishes a measure of the extent of the lesion as well as a sign of its presence. Grunmach finds the cardio-arterial intervals increased only when the compensation fails.

Keyt observes no change in the close-time in mitral stenosis, and Grunmach[3] finds what he does in all other cases, an extraordinary delay with inadequate compensation.

In arterial disease it is agreed that the pulse-wave lingers in an aneurism with yielding walls, and that its velocity is usually greater than normal in sclerosed arteries; but Grunmach holds that the latter fact is due simply to a raised blood-tension, and not to an increased coefficient of elasticity of the arterial wall, and Oppolzer[4] speaks of the pulse as retarded in extreme cases.

[1] Loc. cit. [2] Loc. cit.

[3] Grunmach's views may suitably obtain consideration here. As stated above, he assumes the close-time to be constant and equal to 0.07 sec., which he subtracts from the cardio-arterial intervals to get the aortic-arterial intervals, and gives figures for the latter alone. If we add 0.07 to his aortic-arterial intervals we get his cardio-arterial intervals, which are free from the assumption which he makes. Now he gives the following examples of what he finds in the various valvular lesions with inadequate compensation.

In Health.

Aortic-carotid interval	0.08	Cardio-carotid interval	0.100
Aortic-radial interval	0 09	Cardio-radial interval	0.160
Aortic-dorsalis-pedis interval	0.13	Cardio-dorsalis-pedis interval	0.200

In Aortic Regurgitation.

				Loss.
Aortic-carotid interval	0 066	Cardio-carotid interval	0.130	0.036
Aortic-radial interval	0.135	Cardio-radial interval	0.205	0.045
Aortic-dorsalis-pedis interval	0.170	Cardio-dorsalis-pedis interval	0.246	0.046

In Aortic Stenosis.

				Loss.
Aortic-carotid interval	0.063	Cardio-carotid interval	0.133	0 033
Aortic-radial interval	0.130	Cardio-radial interval	0 200	0.040
Aortic-dorsalis-pedis interval	0.172	Cardio-dorsalis-pedis interval	0.242	0.042

In Mitral Regurgitation.

				Loss.
Aortic-carotid interval	0.064	Cardio-carotid interval	0.134	0.034
Aortic-radial interval	0 128	Cardio-radial interval	0.198	0.038
Aortic-dorsalis-pedis interval	0.174	Cardio-dorsalis-pedis interval	0.244	0.044

It thus appears that, in the opinion of Grunmach, the pulse-wave lost in the short course from the aortic valves to the carotid point four-fifths as much time as in going from those valves to the foot, and this without aneurism or other lesion of the thoracic aorta, but simply in consequence of a lowered general blood-pressure. The observations of Grunmach stand in absolute opposition to those of Keyt and of François-Franck in cases of aortic regurgitation. Keyt gives as a typical case of aortic regurgitation, one in which the cardio-carotid interval was 0.031 and the cardio-radial 0.087.

[4] Krankheiten des Herzens und der Gefässe, pp. 317, 318, 1867.

It is evident that the subject needs working over. This paper is designed as a contribution to the selection of a method of examination, to the establishment of the values of the intervals in question in healthy persons, and to the determination of the extent of variation which they display. It seems as if we might regard the following features of the method to be adopted as the outcome of past experience:

1. The variation of both close-time and pulse-wave velocity must be recognized and determined in each case.

Keyt takes the cardio-arterial intervals, especially the cardio-carotid, as indicating sufficiently closely the variations of the close-time, or, in other words, regards the variations in the aortic-carotid interval as insignificant. This latter period, however, in the healthy cases which I have examined, ranges from 0.023 to 0.038, and it does not seem to me that, in the present condition of our knowledge, it is wise to neglect this 0.015.

2. The velocities should be determined in the same way, and the estimates of distance between the points should be made in the same way in all cases. Since the results are approximate, it is essential for their comparison that they should be obtained by the same method.

3. The point on the pulse-curves which limits the measurement should preferably be the beginning of the rise of the curve, and not its crest, as the former is more free from inertia effects.

4. It is essential that the limiting points be clearly defined and described.

In the following determination of the close-time and pulse-wave velocities, in a few cases of apparently healthy men, the following method has been employed:

The cardio-carotid interval and carotid-radial interval are determined on the right side. The distance from the carotid point to the middle of the right sterno-clavicular articulation, called the clavicular point, is measured as giving the length of the carotid between the carotid point and its origin from the innominate. The distance from the clavicular point to the wrist, measured with the arm extended horizontally in a line with the left shoulder, is taken, and the first measurement subtracted from the second as giving the effective distance corresponding to the carotid-radial interval; and from these the pulse-wave velocity is calculated. This velocity is assumed equal

to that between the aortic valves and the carotid point. The distance from the carotid point to the junction of the lower border of the left third rib with the sternum (called the aortic point) is measured, and from this measurement and the velocity determined as above the aortic-carotid interval is calculated. The aortic-carotid interval is subtracted from the cardio-carotid interval to give the close-time. I know of no more closely approximate method. There are, however, a few points which may be considered. The assumption that the velocity, as obtained, approaches closely to the actual aortic-carotid velocity may be substantiated by the determinations of Keyt. It has been known for about twenty-five years (since Czermak first brought it out) that the velocity is different in different arteries. Keyt made the following determinations in two cases:

	First case.	Second case.
From groin to ankle . . .	1165	1155 cm. per sec.
Down aorta to groin . . .	850	495 " "
Down the arm	805	645 " "

Thus the velocities in the aortic and brachial regions seem sufficiently close to be taken for one another in reference to the short interval between the base of the aorta and the carotid point.

There are three points to be distinguished on the tracings: the beginnings of the radial and carotid pulses and of the ventricular portion of the apex beat. The radial tracing is unambiguous; so, too, the carotid in most cases. Carotid tracings occur in which the application of the exploring tambour is such that there is an initial depression, slightly preceding the point where the rise should begin and obscuring it. I have rejected such tracings. In aortic regurgitation we may have an anacrotic wave, which is regarded as of auricular origin; but with this we have not here to do.

With the cardiac impulse tracing the determination of the point to measure from is more difficult, and the considerations involved more complicated. The point in time from which we wish to measure is the time of closing of the mitral valve. This marks the beginning of the ventricular close-time. While there is extreme difference of opinion as to the point in the cardiographic tracing which marks the closing of the semilunar valves, there seems to be entire harmony in reference to the mitral closure, to the effect that in a typical tracing of a normal

heart such as Fig. 1 it occurs at *a*. But the tracing which we get is far from being always of this frank kind; it may vary in the same individual with the position of the exploring button; in some it is impossible, even after repeated trials of various positions of the exploring button, and various postures of the patient, to get a typical tracing.

Fig. 1.

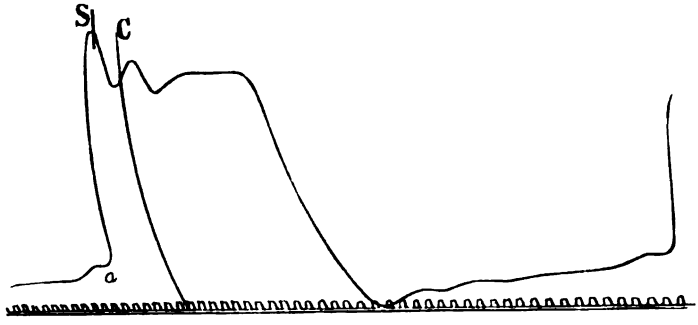


Fig. 2 exhibits four heart-beats of a healthy person as they followed one another in a single tracing, and displays a marked variation in form. This probably depended on the topographical variations incident to the respiratory movement. In Fig. 2 A we have a cardiogram in its most familiar form with I-II as the auricular wave; then follows a marked elevation with four waves—II-III, III-IV, IV-V, and V-VI; and then a longer period of depression, in which the line with some gentle undulations rises slowly, to end with the auricular wave of the next beat. In each of the three following waves we can recognize the same crests and troughs as in the first.

The line C in each figure represents the instant when the carotid pulse begins. The distance of II from C is in all cases practically the same, as I measure it, being in A 113, in B 124, in C 126, in D 121. At A the cylinder was moving too slowly and the carotid line was too imperfect to be as reliable as in the other three curves. Thus the position of the point of mitral closure seems to be well preserved in all the four curves, even though they include such marked deviations from form A (which we may regard as the typical cardiogram) as are exhibited by form D.

There is another interval which appears to keep constant through

all these changes of form: this is, that between II and the crest between V and VI, which in B, C, and D equals 0.306 second. I omit A on account of the difficulty of accurately determining the time. I have measured the distances between the successive troughs, and, as appears in the following table, they are very far from constant:

	A	B	C	D
I-II	0.072	0.070	0.070	0.091[1]
II-III	0.103	0.097	0.086	0.062
III-IV	0.076	0.099	0.105	0.130
IV-V	0.103	0.103	0.103	0.107
V-VI	0.171	0.182	0.144	0.196

Martius (loc. cit.) freely admits that the amplitudes of the waves making up the cardiogram are of no definite significance. If now the wave-lengths vary independently of intracardiac events, the possibility of a general interpretation of the cardiogram is out of the question. But it remains possible that in cardiograms of a certain form the various turning-points might be found to have definite relations to such events.

Evidence has been offered by Martius (loc. cit.) to show that the crest between II and III marks the opening of the semilunar valves, and this view has been confirmed by the independent observations of Edgren (loc. cit.). I think this is approximately true for fairly typical cardiograms. Martin,[2] rather curiously, finds a discrepancy between the results of the observers named and Rolleston,[3] who puts the point of opening of the aortic valves in the middle of the main upstrokes of his tracings. But Martin overlooks the fact that Rolleston's tracings are of endocardial pressure, while those of Martius and Edgren are of the apex-beat. In the latter, the pressure against the thoracic wall increases for a certain time, in consequence of the increasing tension of the heart-wall and concomitant changes of form. This pressure diminishes after the opening of the semilunar valves, not because the intraventricular pressure diminishes, but because the heart grows smaller, and, therefore, tends to retreat from the chest-walls. The

[1] Not well marked.

[2] Universal Annual of the Medical Sciences, 1888, vol. v. p. 315. Article, Physiology, by H. Newell Martin and W. H. Howell.

[3] H. D. Rolleston, Observations on the Endocardial Pressure Nerve. Journal of Physiology, vol. viii. p. 235, 1887.

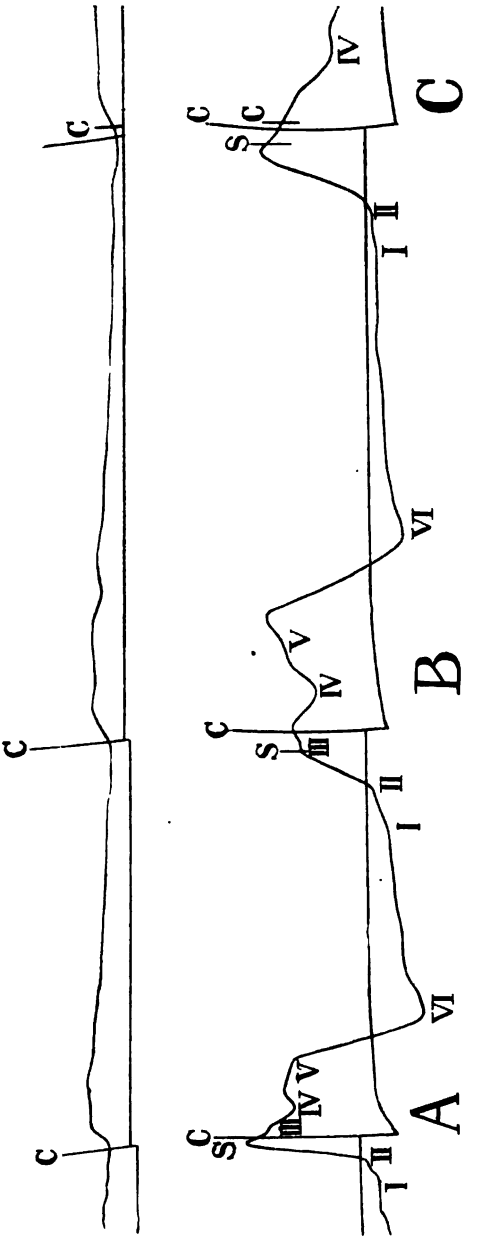
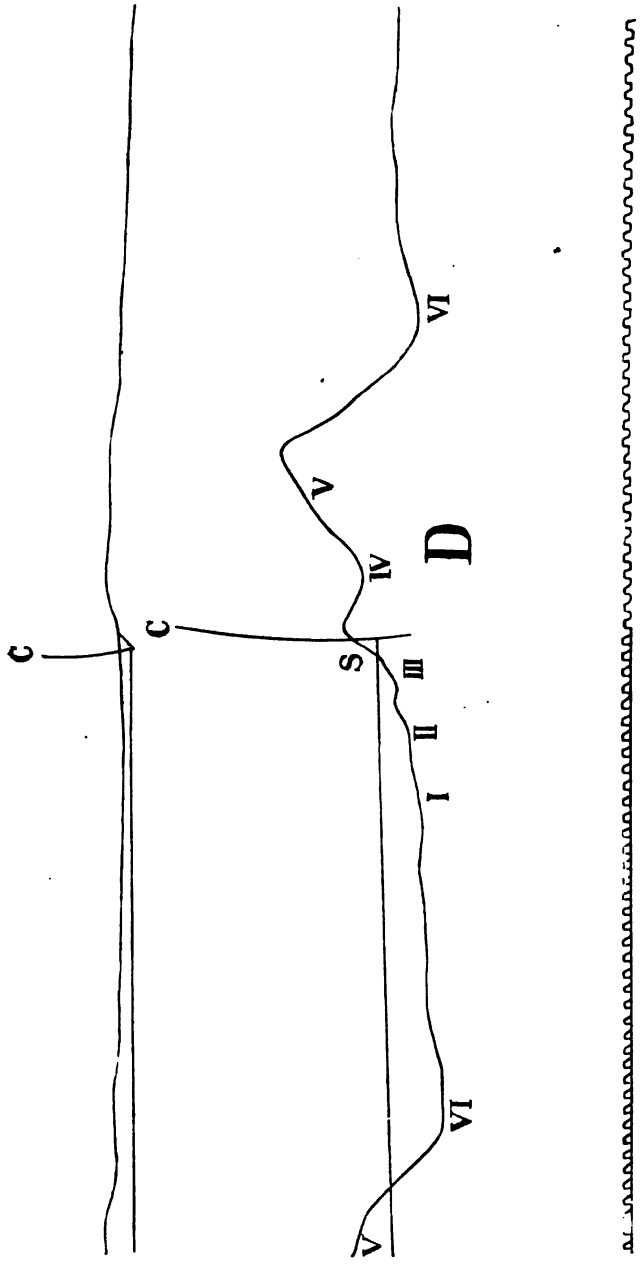


FIG. 2.


c
c
s
D
I
II
III
IV
V
VI
V
VI

intra-ventricular pressure *must* increase after the opening of the semi-lunar valves, else the equal aortic tension would have no way of regaining its rhythmic maximum.

If this first ventricular crest marked accurately in all cases the opening of the aortic valves, we should be able to get the close-time directly from the cardiogram. How little the latter can be trusted in that way, will appear from the tracing above. I have drawn in at S (by measuring from the carotid pulse, point C, back 0.031 sec., which was the aortic-carotid interval in this case) the point where the aortic valves open, and it appears that its distance from the position assigned to it by Martius and Edgren may be very considerable. In the following determinations, then, the cardiographic curve is used only for determining the beginning of systole.

I may add that the curves were synchronized by revolving the drum after the tracing had been taken, and making the tambours mark synchronous lines in the appropriate places, as is now the custom. The time-marker made 48.6 double vibrations per second. The rate of speed was about one-third greater than that of the tracing already given, the original tracing having been photographed down to three-quarters. I give the individual determinations in detail, in order to furnish an idea of the amount of variation in the determinations in each case, and the number of such determinations made. The equality of the transmission-times between the exploring and receiving tambours, with air-transmission, was tested by repeated trials, and made accurate. The hearts and arteries were carefully examined, and no indications of disease were found. The subjects were all, to the best of my knowledge, in good health in every way, and with the exception of VI, a light muscle-worker, were professional men and schoolboys.

I.—Age thirteen.

	Cardio-carotid Intervals.	Cardiac Cycles
Paper No. 1:	0.103, 0.109, 0.093. Av. 0.102	0.99, 0.80, 1.03. Av. 0.94
Paper No. 2:	0.82, 0.76, 0.82. Av. 0.80	0.95, 0.76, 0.72. Av. 0.81
	Average of both papers, 0.091	+0.88

Carotid-radial intervals: 0.084, 0.078, 0.076, 0.082, 0.070, 0.066. Average, 0.076.

Carotid-clavicular distance, 8.5 cm.

Carotid-aortic distance, 16.5 cm.

Clavicular-radial distance, 50.7 cm.

50.7 — 8.5 = 42.2

0.076 : 1 :: 42.2 : 555 = pulse-wave velocity down the arm.

555 : 16.5 :: 1 : 0.030 = carotid-aortic interval.

0.091 — 0 030 = 0.061 = ventricular close-time.

If we compare the cardio-carotid intervals in the two papers and the cardiac cycles, it will be observed that the longer cardiac cycle corresponds to the longer cardio-carotid interval.

II.—Age thirty.

	Cardio-carotid Intervals.	Cardiac Cycle
Paper No. 1:	0.099, 0.101, 0.104. Av. 0.101	0.82, 0.80. Av. 0.81
Paper No. 2:	0.111, 0.109, 0.110. Av. 0.110	0.75. Av. 0.75
	Average of both papers, 0.105	0.78

Carotid-radial intervals: 0.086, 0.107, 0.082, 0.093, 0.095. Average, 0.093.

Carotid-clavicular distance, 9.

Carotid-aortic distance, 19.

Clavicular-radial distance, 65.

65 — 9 = 56, carotid-aortic distance.

0.093 : 1 :: 56 : 602 = pulse-wave velocity.

0.602 : 19 :: 1 : 0.031 = carotid-aortic time.

0.105 — 0.031 = 0.074 = close-time.

In the two papers in this case the cardio-carotid intervals vary inversely with the cardiac cycles.

III.—Age twenty-four.

Cardio-carotid intervals: 0.091, 0.094, 0.091. Average, 0.092.

Cardiac cycle, 0.84.

Cardio-radial intervals: 0.082, 0.082, 0.093. Average, 0.086.

Carotid-clavicular distance, 11.

Carotid-aortic distance, 21.5.

Clavicular-radial distance, 76.

76 — 11 = 65.

0.086 : 1 :: 65 : 756 = pulse-wave velocity.

756 : 21.5 :: 1 : 0.028 carotid-aortic distance.

0.092 — 0.028 = 0.064 = close-time.

Arterial pressures by sphygmanometer:

Right radial, 117

Left radial, 117

Right temporal, 64

Left temporal, 64

IV.—Age thirty-six.

Cardio-carotid Intervals.	Cardiac Cycle.
0.099, 0.101, 0.105, 0.103. Av. 0.102	1.03, 1.05, 1.05. Av. 1.04

Carotid-radial intervals: 0.85, 0.86. Average, 0.855.
Carotid-clavicular distance, 12.
Carotid-aortic distance, 21.
Clavicular-radial distance, 66.
66 — 12 = 54.
0.0855 : 1 :: 54 : 632 = pulse-wave velocity.
632 : 21 :: 1 : 0.033.
0.102 — 0.033 = 0.69 = close-time

V.—Age twenty-five. Apex-beat not obtained.
Carotid-radial intervals: 0.062, 0.062, 0.062, 0.060, 0.068, 0.072. Av. 0.065.
Carotid-clavicular distance, 10.5.
Clavicular-radial distance, 66.5.
66.5 — 10.5 = 56.
0.065 : 1 :: 56 : 862 = pulse-wave velocity.
Arterial tensions with sphygmanometer:
Right radial, 130
Left radial, 130
Right temporal, 70
Left temporal, 80

VI.—Age forty-three.

	Cardio-carotid Intervals.		Cardiac Cycle.	
Paper No. 1:	0.115, 0.103, 0.113, 0.111.	Av. 0.111	1.03, 0.90, 0.92.	Av. 0.95
Paper No. 2:	0.119, 0.119	Av. 0.119	1.10, 0.99	Av. 1.05
	Average of two papers,	0.115		1.00

Carotid-radial intervals: 0.076, 0.072, 0.082, 0.082, 0.070, 0.082, 0.076. Av. 0.077.
Carotid-clavicular distance, 12.
Carotid-aortic distance, 21.5.
Clavicular-radial distance, 66.
66 — 12 = 54.
0.077 : 1 :: 54 : 701 = pulse-wave velocity.
701 : 21.5 :: 1 : 0.031 = carotid-aortic time.
0.115 — 0.031 = 0.084 = close-time.
Here the cardio-carotid intervals vary directly with the cardiac cycles.

VII.—Age seven.

	Cardio-carotid Intervals		Cardiac Cycle.
Paper No. 1:	0.093, 0.099	Av. 0.096	0.74
Paper No. 2:	0.105, 0.107, 0.099, 0.103,	Av. 0.104	0.82
	Average of both papers,	0.100	0.78

Carotid-radial intervals: 0.082, 0.082, 0.082. Average, 0.082.
Carotid-clavicular distance, 5.

Carotid-aortic distance, 11.5.
Clavicular-radial distance, 43.
43 — 5 = 38.
0.082 : 1 :: 38 : 463 = pulse-wave velocity.
463 : 11.5 :: 1 : 0.025 = carotid-aortic interval.
100 — 0.025 = 0.075 = close-time.
Here, too, the cardio-carotid intervals vary directly with the cardiac cycles.

VIII.—Age thirty-three. Apex-tracing not obtainable.

Carotid-radial Intervals.

Paper No. 1: 0.082, 0.084, 0.093. Average, 0.086
Paper No. 2: 0.076, 0.076, 0.076. Average, 0.076
Average of both, 0.081

Carotid-clavicular distance, 11.
Clavicular-radial distance, 69.5.
69.5 — 11 = 58.5.
0.081 : 1 :: 58.5 : 722, pulse-wave velocity.

IX.—Age forty-six. Apex-tracing not obtainable.
Carotid-radial intervals: 0.062, 0.064, 0.064, 0.066, 0.062. Average, 0.064.
Carotid-clavicular distance, 11.5.
Clavicular-radial distance, 65.5.
65.5 — 11.5 = 54.
0.064 : 1 :: 54 : 844 = pulse-wave velocity.

X.—Age twenty-seven.

	Cardio-carotid Intervals.		Cardiac Cycle.	
Paper No. 1:	0.113, 0.113, 0.109	Av. 0.112	0.91, 0.92	Av. 0.92
Paper No. 2:	0.105, 0.105, 0.105	Av. 0.102	1.02, 0.98	Av. 1.00
Paper No. 3:	0.109, 0.111, 0.093, 0.093.	Av. 0.102	0.97, 1.01, 1.04.	Av. 1.01
	Average of these papers,	0.106		0.98

Carotid-radial intervals: 0.117, 0.99, 0.107, 0.093, 0.091, 0.82. Average, 0.098.
(The tracings were very low, and the initial point hard to determine.)
Carotid-clavicular distance, 9.5.
Carotid-aortic distance, 21.5.
Clavicular-radial distance, 65.5.
65.5 — 9.5 = 56.
0.098, : 1 :: 56 : 571 = pulse-wave velocity.
571 : 21.5 :: 1 : 0.038 = aortic-carotid time.
0.106 — 0.038 = 0.068 = close-time.
Pulse tension:
Right radial, 120
Left radial, 110
Right temporal, 64
Left temporal, 65

The cardio-carotid time seems here to vary inversely with the cardiac cycle, if we compare the three papers; and so it does in paper No. 3, if we contrast the beginning and end of the paper.

XI.—Age forty-six.

	Cardio-carotid Intervals.		Cardiac Cycle.	
Paper No. 1:	0.086, 0.086, 0.091.	Av. 0.088	0.92, 0.85.	Av. 0.88
Paper No. 2:	0.091	Av. 0 091	0.82	Av. 0.82
Paper No. 3:	0.082, 0.082	Av. 0.082	0.96, 0.96.	Av. 0.96
Paper No. 4:	0.099, 0.103	Av. 0.101	0.96, 0.96.	Av. 0.96
Paper No. 5:	0.099, 0.099, 0.093, 0.101.	Av. 0.098	0.86, 0.86, 0.85.	Av. 0.86
	Average of five papers,	0.092		0.90

Carotid-radial interval: 0.088, 0.082, 0.082. Average, 0.084.
Carotid-clavicular distance, 10.05.
Carotid-aortic distance, 21.
Clavicular-radial distance, 66.
66 — 10.5 = 55.5.
0.084 : 1 :: 55.5 : 661 = pulse-wave velocity.
661 : 21 :: 1 : 0.032 = carotid-aortic interval.
0.092 — 0.32 = 0.060 = close-time.
Pulse tension:
Right radial, 126
Left radial, 126
Right temporal, 82
Left temporal, 73

The variations of the cardio-carotid interval in this case seem to behave irregularly with reference to those of the cardiac cycle.

XII.—Age twenty-five.

	Cardio-carotid Intervals.		Cardiac Cycle.	
Paper No. 2:	0.103, 0.109, 0.113.	Av. 0.108	1.13, 1.12, 1.14.	Av. 1.13
Paper No. 3:	0.082, 0.084.	Av. 0.083	1.03, 1.05, 1.02.	Av. 1.03
	Average of two papers,	0.096		1.08

Carotid-radial interval: 0.095, 0.093, 0.088, 0.091. Average, 0.92.
Carotid-clavicular distance, 10.
Carotid-aortic distance, 19.5.
Clavicular-radial distance, 65.5.
65.5 — 10 = 55.5.
0.092 : 1 :: 55.5 : 603 = pulse-wave velocity.
603 : 19.5 :: 1 : 0.032 = carotid-aortic interval.
0.096 — 032 = 0.064 = close-time.

Pulse tension:

Right radial, 118
Left radial, 105
Right temporal, 68
Left temporal, 65

The cardio-carotid intervals in this case seem to vary directly with the cardiac cycles.

XIII.—Age fifteen.

Cardio-carotid Intervals.	Cardiac Cycle.
0.095, 0.095, 0.099, 0.101. Av. 0.097	0.88, 0.96, 0.97. Av. 0.94

Carotid-radial intervals: 0.109, 0.107, 0.109. Average, 0.108.
Carotid-clavicular distance, 10.5.
Carotid-aortic distance, 18.5.
Clavicular-radial distance, 62. 5.

61.5 — 20.5 = 52.
0.108 : 1 :: 52 : 482 = pulse-wave velocity.
482 : 18.5 :: 1 : 0.038 = carotid-aortic interval.
0.097 — 0.038 = 0.059 = close-time.

Pulse tension:

Right radial, 92
Left radial, 107
Right temporal, 66
Left temporal, 70

XIV.—Age forty.

Cardio-carotid Intervals.	Cardiac Cycle.
0.124, 0.113, 0.124. Average, 0.120.	1.15, 1.13. Average, 1.14.

Carotid-radial intervals: 0.062, 0.062, 0.072. Average, 0.065.
Carotid-clavicular distance, 10.5.
Carotid-aortic distance, 20.
Clavicular-radial distance, 66.

66 — 10.5 = 55.5.
0.065 : 1 :: 55.5 : 854 = pulse-wave velocity.
854 : 20 :: 1 : 023 = carotid-aortic interval.
0.120 — 0.023 = 0.097 = close-time.

Pulse tension:

Right radial, 116
Left radial, 119
Right temporal, 65
Left temporal, 67

XV.—Age forty-six.

	Cardio-carotid Intervals.		Cardiac Cycle.	
Paper No. 1:	0.101, 0.109	Av. 0.105	0.75	Av. 0.75
Paper No. 2:	0.107, 0.111	Av. 0.109	0.68, 0.70.	Av. 0.69
Paper No. 3:	0.097, 0.099, 0.093.	Av. 0.096	0.69, 0.70.	Av. 69.5
Paper No. 4:	0.093, 0.101	Av. 0.097	0.77, 0.75.	Av. 0.76
	Average of four papers,	0.102		0.72

Carotid-radial intervals: 0.074, 0.074, 0.082. Average, 0.077.
Carotid-clavicular distance, 10.5.
Carotid-aortic distance, 21.5.
Clavicular-radial distance, 73.5.
$0.73.5 - 10.5 = 62$ = virtual carotid-radial distance.
$0.077 : 1 :: 62 : 805$ = pulse-wave velocity.
$805 : 21.5 :: 1 : 0.027$ = carotid-aortic interval.
$0.102 - 0.027 = 0.075$ = ventricular close-time.
Pulse tension:
Right radial, 103
Left radial, 113
Right temporal, 82
Left temporal, 78

In this case again there seems to be no definite relation between the variations of the cardio-carotid time and the cardiac cycle.

The results are summed up in the following table:

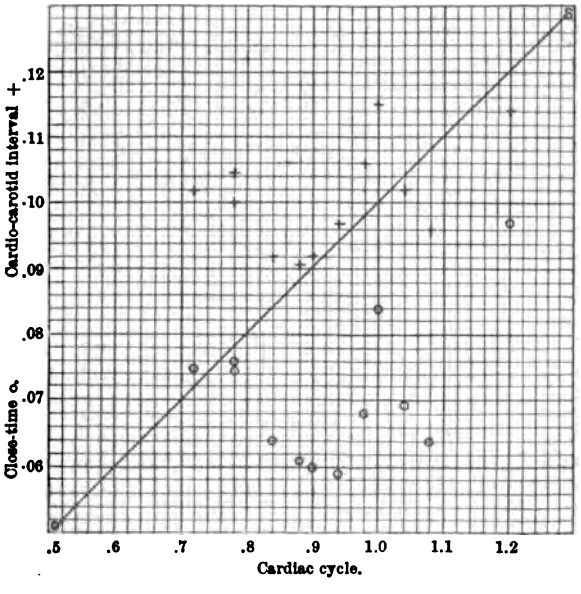
Number.	Age.	Cardiac cycle.	Cardio-carotid interval.	Carotid-radial interval.	Aortic-carotid interval.	Close-time.	Velocity.	Average temporal tension.	Average radial tension.	Ratio of cardiac cycle to cardio-carotid interval.	Ratio of cardiac cycle to close-time.
I.	13	0.88	0.091	0.076	0.030	0 061	555	...	.....	9.7	14.4
II.	30	0.78	0.105	0.093	0.031	0.074	602	...	.....	7.4	10 5
III.	24	0.84	0.092	0.086	0.028	0.064	756	64	117	9.1	14.7
IV.	36	1.04	0.102	0.0855	0.033	0 069	632	...	.....	10.2	15.1
V.	25	...	.....	0.065	.....	.....	862	75	130	.....	.....
VI.	43	1.00	0.115	0.077	0.031	0.084	701	...	.....	8.8	11 9
VII.	7	0.78	0.100	0.082	0.025	0.075	463	...	.....	7.8	10.4
VIII.	33	...	.....	0.081	.....	.....	722	...	.....	.....	.....
IX.	46	...	.....	0.064	.....	.....	844	...	.....	.....	.....
X.	27	0.98	0.106	0.098	0.038	0 068	571	65	115	9.2	14.5
XI.	46	0.90	0.092	0.084	0.032	0.060	661	78	126	9.8	15.0
XII.	25	1.08	0.096	0.092	0.032	0.064	603	67	112	11.3	16.9
XIII.	15	0.94	0.097	0.108	0.038	0·059	482	68	100	9.7	15 9
XIV.	40	1.20	0.114	0.062	0.023	0.097	854	66	118	10.5	12.4
XV.	46	0 72	0.102	0.077	0.027	0 075	805	...	.....	7.1	9.6

In this comparatively small number of healthy cases, then, the close-time, determined by the same observer by the same method, and with all possible care, varied from 0.059 to 0.097, and with a larger number of

cases these limits would doubtless move a little further apart. The effect of the length of the cardiac cycle upon the length of the close-time demands attention. Keyt says that the length of the cardiac cycle is ten times that of the cardio-carotid interval, not with absolute exactness, but still with sufficient accuracy to inform us whether a given interval is abnormal or not. The relation really varies in my cases from 7.1 to 11.3, which is almost exactly the same as from 0.059 to 0.097. Nor does the ratio of the close-time to the cardiac cycle fare any better; that varies from 9.6 to 16.9. I do not mean to imply that quickened pulse-rate has no effect on the close-time, but merely that that factor has an influence so small that its effect is swamped by that of other factors, and that if we think Keyt's rule is worth applying, we must allow that deviations from 10 as great as 11.3, and as small as 7.1, are within the limits of health.

I have plotted these results, and the want of regularity in the variations will be apparent there. (See Fig. 3.) The abscissas represent

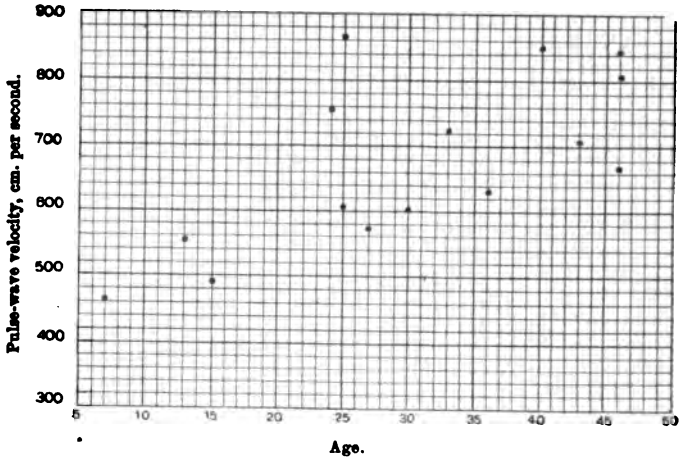
FIG. 3.



the length of the cardiac cycles, the ordinates the length of the cardio-carotid intervals (+) and of the close-times (o). If Keyt's rule were true, the cardio-carotid intervals would lie on the slant line *os*.

The pulse-wave velocities as plotted in Fig. 4, rise with a fair steadiness with the age, from between 400 and 500 at the age of seven,

FIG. 4.



to between 800 and 900 between forty and fifty. How far there is a liability to wander from the main path the plotting exhibits. Whether there is any significance in the high velocity of V., time must show. There is no other abnormal feature discoverable at present in that person. It will be noticed that the pulse tension was high.

DISCUSSION.

DR. G. BAUMGARTEN: The line of research followed by Dr. Thacher is of great importance in the interpretation of the pulse phenomena. It is very desirable to know accurately the rate of transmission of the pulse-wave, as well as the exact duration of the presphygmic interval. But it seems to me impracticable[1] to discuss the subject intelligently until the substance of the communication and the values arrived at are before us in detail.

[1] Dr. Thacher having presented to the Association only a verbal *résumé* of his paper.

A PIN IMBEDDED IN A WOMAN'S HEART.

By GEORGE L. PEABODY, M.D.,
PROFESSOR OF MATERIA MEDICA AND THERAPEUTICS IN THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

THE patient from whose body the heart that I show you was removed, died in the New York Hospital on the 26th of last May. Unfortunately, she was under observation only a few hours, and only an incomplete history of her could, therefore, be obtained.

She was twenty-nine years of age, a native of Ireland, married, and a nurse by occupation. She said that one brother had rheumatism, but she gave no other morbid family history. She had never had rheumatism, malarial fever, or syphilis. She confessed to a very moderate alcoholic habit. On her admission she was suffering greatly from dyspnœa, and was unable to give a circumstantial account of her illness. Five years previously she had given birth to a girl, her labor having been uncomplicated. Five months previously she had again been confined, and up to the time of her admission was acting as a wet-nurse. She said that for some time previously she had passed less urine than normal, that it was high colored, and that from time to time her feet had been swollen. For four months previously she had been short of breath, had suffered from palpitation, vertigo, muscæ volitantes, tinnitus aurium, morning nausea and vomiting, constant swelling of the feet and abdomen. Her appetite had been poor, her bowels regular.

At the time of her admission on the 25th of May, 1888, at 4.30 P. M., her temperature was 100.8°, her respirations 44, and her pulse 130. A specimen of her urine obtained immediately, was of a yellow color, acid reaction, 1020 specific gravity, contained a large amount of albumin, many hyaline and epithelial casts, and pus cells.

On physical examination, both pleuræ were found to contain fluid as high as the angles of the scapulæ, and there were a great many moist râles all over, both behind and in front. The heart-sounds were very feeble, and heard with difficulty. No murmur could be distinguished. There was œdema of the feet and legs.

She was given acetate of potash and infusion of digitalis, and passed a fairly comfortable night. The next morning she seemed to be doing fairly well, when she suddenly experienced great precordial pain, distress, and

dyspnœa, which latter was intense. She was given morphine subcutaneously, but she continued to suffer, and died in half an hour, at 10.45 A. M.

An *autopsy* was made on the same day, at 2 P. M. There was general anasarca; rigor mortis was well marked; the peritoneum was normal; the diaphragm was at the level of the fourth rib on the right side and of the sixth rib on the left side. There were 1300 cubic centimetres of serum in the left pleural cavity, and half as much in the right. There were no pleuritic adhesions.

Heart.—The pericardium was normal; the heart was very slightly increased in size; the wall of the left ventricle was to a slight extent thickened; the cusps of the mitral valve were somewhat thickened, and there was a perceptible degree of stenosis of the mitral valve. The other valves were normal. The muscular tissue was soft, flabby, yellowish, and in places contained fat. A black, or blackened, pin, the top of the head of which was free in the left ventricle, was seen piercing one of the papillary muscles attached to the anterior segment of the mitral valve, and penetrating the wall of the ventricle obliquely to the depth of three centimetres. The point of the pin was distant five millimetres from the external surface of the heart. The pin seemed eroded, and was broken by accidental contact with the scissors without, however, becoming displaced. There was no evidence of any *recent* inflammation in this neighborhood, but the endocardium in the neighborhood of the protruding head of the pin was greatly thickened and snow-white, and firmly adherent to the edge of the head. The endocardium at a corresponding point on the opposite surface of the ventricle was also thickened. This latter area of thickened endocardium measured one and a half centimetres in diameter, and could be more distinctly seen when the specimen was fresh than it can be now. The shaft of the pin just below the head was unevenly thickened.

Following the course of the pin, there can plainly be seen a linear cicatricial depression in the papillary muscle. A most careful examination of the heart failed to reveal the route by which the pin had travelled to its present location.

The *œsophagus*, *stomach*, *pericardium*, and *great vessels* were examined with great care and found to be uninjured. A microscopical examination of many parts of the heart showed fat in the muscular fibres, but not as generally as had appeared to the naked eye. The right ventricle contained more fat than did the left.

The *spleen* was normal.

The *kidneys* were normal in size; they were intensely congested, their cortices were swollen, their markings were not distinct. The microscope showed the epithelium of the kidneys to be generally very granular, and also showed fat in the epithelial cells of some of the convoluted tubes.

The *stomach* was found, by the microscope, to have an enormous amount of fat in its tubes, generally confined to the lower two-thirds of them, but in some of them extending up to the surface of the mucous membrane. The fat was in the shape of very small globules; the epithelium was very granular.

The *liver* was very fatty.

The *bladder*, *ovaries*, and *uterus* were normal.

I show the specimen to the Association as a pathological curiosity, and not in expectation that anything can be learned from it, though it is certainly interesting that even so small a foreign body as the upper surface of the head of a pin should have been for so long a time free in the cavity of the ventricle without producing thrombosis.

The pin had evidently been in its present situation for a long time, and in all probability had entered the body by the mouth. Whether it was causative in the production of the heart lesions and secondarily of the kidney and other lesions can only be a matter of conjecture in the absence of a definite history. I have in vain endeavored to obtain such a history from the husband and employer of the woman. A letter from the latter gentleman—a physician—states that during the week preceding her admission to the hospital she suffered occasionally from severe attacks of dyspnœa.

THE RELATION BETWEEN TROPHIC LESIONS AND DISEASES OF THE NERVOUS SYSTEM.[1]

By E. C. SEGUIN, M.D.,
PRESIDENT OF THE AMERICAN NEUROLOGICAL ASSOCIATION, ETC.

THE question chosen by the Council of the Association for discussion, viz.: "*The relation between trophic lesions and diseases of the nervous system, excluding changes within the central nervous system itself,*" is one which brings the critic face to face with an enormous accumulation of more or less well-observed, widely diverse, and not necessarily correlated clinical and experimental facts or data.

The very terms of the question involve to my mind a petitio principii: for the essential query in any review of the data is whether the lesions referred to are really trophic, in nervous causal relation.

Your referee has been informed that "permanent vaso-motor changes in so far as they can be shown to influence nutrition" may or should be included in the discussion. This I consider a wholly different field of physio-pathology and one whose introduction into the discussion would only tend to obscure, complicate, and indefinitely extend the search after the true relation between trophic lesions and nervous diseases; and I shall consequently omit it from my remarks.

We must also insist upon a strict definition of the word lesion, as meaning a positive histological alteration in tissue, thus excluding retarded or imperfect development, simple quantitative reduction of tissues, and alterations in local circulation and calorification (as observed in cerebral and spinal paralysis occurring before the full growth of the body, and occasionally after it).

Thus simplified, the question may be re-stated in the following terms: *What are the lesions which may be supposed to be directly produced by disease of the nervous system (brain, spinal cord, and nerves); and what is the essential causal relation between the two factors?*

[1] Opening a discussion in conjoint meeting with the American Physiological Society.

To attempt to enumerate separately in a systematic manner all the lesions of the non-nervous tissues which have been observed clinically and experimentally, and ascribed to a direct morbid nervous influence (positive or negative), would be an extensive work, far beyond the time-limits of this discussion.

Such "trophic lesions" have been described as occurring in almost all the non-nervous organs and tissues, internal and external. We find medical literature filled with examples of such lesions in the cutaneous tissue and appendages, in the muscular tissue, in bones and articulations, in vascular walls, in parenchymatous organs, and in internal epithelial structures. In other words, wherever nerve fibres terminate, and even where none can be demonstrated (as in cartilage), such lesions have been described.

Some simple mode of classification of these data must be chosen to facilitate discussion, and looking at the subject from the standpoint of the pathologist and practical physician I would suggest the following:

First Class.—"Trophic lesions" occurring in parts whose sensibility is more or less reduced by the nervous disease, and which are exposed to the action of traumatic and infectious influences.

This class includes by far the largest number of the data, such as cutaneous ulcerations, fall of nails and hair, altered appearance of skin and nails, articular changes (arthropathies), fractures of bones, deep eschars, necrosis of digits, and most of the lesions observed in the hollow viscera lined by epithelia. To put it in another way, this class includes the various lesions observed in the course of posterior spinal sclerosis, injuries of the spinal cord, forms of "myelitis" so-called, and injuries to nerve-trunks. Speaking before this audience it is useless to specify further the "trophic lesions" referred to.

Second Class.—Trophic lesions occurring in deeper parts, not exposed to bacterial infection, and upon which traumatic influences cannot be demonstrated to act: in other words, the apparently spontaneous trophic lesions. This class is made up of the muscular atrophies occurring in the course of nervous diseases, and of a limited number of cutaneous lesions. Possibly some alterations in glandular function, due to nerve lesion, may belong to this class. Clinically, these lesions are met with after nerve section, in the course of neuritis, and of disease affecting primarily or at least chiefly the ventral cornua

of the spinal cord, and their homologues in the cephalic prolongation of the spinal axis.

With respect to the first class of so-called trophic lesions, a careful study of the conditions under which they arise, and a comparison of them with the similar peripheral lesions which present as complications at the close of some non-nervous diseases with tendencies to asthenia and inanition, make it somewhat doubtful whether they can rightly be considered as direct results of suppressed or perverted nervous action. Even the strongest partisan of the truly dystrophic nature of these lesions admits that extraneous influences (as trauma and bacterial infection) play a certain, though wholly secondary, part in their genesis. But other observers hold an opposite extreme view, and claim that the real or efficient causes of the lesions are trauma and infection acting upon parts which have lost their automatic defence through anæsthesia, and whose circulation and general nutrition are lowered, but not specifically altered, by disease and inertia.

This second view is, I must say, supported by negative evidence, experimental and clinical, of such importance that it needs to be stated.

(*a*) Ulceration of the cornea and even panophthalmitis are results of experimental and pathological injury to the trigeminus nerve, more especially of its ophthalmic branch and of the Gasserian ganglion. These ocular lesions are fully described in text-books, and are generally looked upon as typical trophic changes due to the nerve disease. Yet thirty years ago H. Snellen[1] and M. Schiff[2] separately demonstrated by the simple experiment of sealing the eye by sewing the edges of the lids or by fastening one of the animal's ears over it, that ulceration of the cornea could be prevented after section of the trigeminus. At some time prior to 1872 von Gudden[3] proved the same thing by a beautiful experiment. He took newly born rabbits and produced perfect closure of the eyelids by an operation (artificial ankyloblepharon). When the wounds were healed, and the eyes absolutely sealed, he cut the trigeminus nerve by the usual intracranial method. Upon opening the eyelids from eight to fifteen days

[1] H. Snellen, De invloed der zennwen, op. de ontsteking. Dissert. Utrecht, 1857. Also in Archiv f. d. Hollandische Beiträge, zur Natur. Heilkunde, Bd i. 3, p. 206 (1857).

[2] M. Schiff, in Canstatt's Jahresbericht, i. p. 121, 1857.

[3] Von Gudden, cited by Kondracki in his thesis, Ueber die Durchschneidung des Nervus Trigeminus, Zürich, 1872.

after the nerve-section he invariably found the cornea normal. These experiments make it clear that the nerve lesion is not the real or efficient cause of the corneal changes in the usual experiments, and probably not in human cases of disease of the fifth nerve.

(*b*) Section and other injuries of nerve-trunks have long been known to be followed by so-called trophic lesions in the distal parts supplied by the injured nerve. Changes in the skin and hairs, falling of the nails, ulceration, and even extensive necrosis or gangrene have been elaborately described in animals and in man. Yet it is nearly forty years since Brown-Séquard[1] showed that (in animals) if the parts supplied by the injured nerve be kept perfectly clean and protected from traumatic influences, ulcerations, etc., did not ensue. He also demonstrated that wounds made in parts supplied by an injured nerve-trunk healed as well as wounds made elsewhere. These experiments (confirmed by numerous observers) show that the nerve injury in such experiments or cases is not the true efficient cause of the ulcerations, etc., and also (what is fully as important) that the nutritive functions which go to repair wounds are fully active in anæsthetic and paralyzed parts. In the practice of medicine we have frequent occasion to apply these data, in the prevention and treatment of ulceration, bed-sores, etc., by mechanical and antiseptic measures, after injuries to nerves or to the spinal cord, as well as in cases of paraplegia. In human cases of section of sensory nerves, while certain quantitative changes in the anæsthetic area are apparently inevitable, actual histological lesions can, I believe, be indefinitely prevented by guarding against traumatic influences.[2] As regards bed-sores in paraplegia, you have probably all seen them show healthy reparative action while the spinal disease was growing worse.

(*c*) It is a remarkable fact that such lesions as perforating ulcer, arthropathies, fractures, etc., which occur in the course of posterior spinal sclerosis and other affections, are extremely rare in patients whose circumstances enable them to avoid over-exertion in the later stages of the disease, and to receive every needed care. This certainly would point to traumatism as a potent factor in the production of the so-called trophic lesions of tabes.

[1] Brown-Séquard, Gazette Médicale, p. 880, 1849.

[2] Except some lesions of the second class which are unpreventable, though it would seem not invariable results of nerve injuries.

(*d*) Cystitis was until a comparatively recent time considered one of the symptoms of myelitis and of injury to the spinal cord; though I suppose most of us to-day would speak of it as a complication preventable by the use of aseptic catheters introduced with the greatest care.

The negative demonstrations and arguments to the effect that the greatest number of the most formidable of the so-called trophic lesions of the first class are preventable and curable, appear to my mind almost overwhelming proof that the efficient cause of these lesions is not a suppression or perversion of nervous action or influence.

Consequently I would refuse the name of trophic lesions to the phenomena embraced in the first class of data.

We are now brought to the study of the second class of "trophic lesions," those in which extraneous or traumatic causes cannot be shown to act. It is perhaps in the study of these that the problem of the relation between the nervous disease and the lesion can be best approached.

The most typical lesions of the second class are muscular atrophy with degeneration, and the cutaneous affection known as herpes or zona. Probably other so-called skin diseases belong to this group, but full demonstration of their nervous origin is wanting.

(*a*) The natural history of neuro-muscular atrophic degeneration is well known to all of you. Within a few days after section of a nerve-trunk, or after destruction of the ventral ganglion cells with which a nerve-trunk is associated in the spinal axis (clinically, in cases of nerve injury, neuritis, poliomyelitis, chronic degeneration of ventral ganglion cells, etc.), the nerve fibres distal of the point of injury or disease, and all the muscles innervated by the fibres, lose certain properties known as conductibility and irritability, react abnormally to electrical stimulation, and if examined with the microscope show distinct and invariable alterations. Later, the affected muscles undergo a marked reduction of volume.

In adult animals, after certain lesions, an extreme degree of atrophy is established and persists. In young animals, after certain lesions (simple nerve injuries and neuritis more especially), a process of regeneration sets in which in a few months leads to return of a normal anatomical state of the nerves and muscles, and to renewed functional activity. In some cases there are also more diffused changes produced,

arrest of development of parts, quantitative modifications which should not be confounded with actual lesions. What I wish to emphasize is that we have here to deal with *qualitative or histological changes in nerves and muscles, which occur with fatal necessity when the cause has acted*, and which are demonstrable by microscopic examination and electrical tests (reaction of degeneration). Further, that these lesions are unpreventable, and in one sense incurable; no traumatic or infectious influence can be traced in their genesis, and no amount of care or any form of treatment will prevent the appearance or thwart the evolution of the changes.

(*b*) The so-called herpetic lesions of the skin. The vesiculo-pustular affection appears all at once or in successive crops upon areas of skin supplied by one or more of the cerebro-spinal nerves; the distribution of the primary eruption and of the subsequent scars corresponding exactly with nerve territories. Hence the names for varieties of herpes, such as H. frontalis, H. cornese, H. progenitalis, H. intercostalis, etc. Along with the eruption there are not rarely subjective symptoms of nerve irritation, such as burning prickling, pain, or numbness. In some cases pain (neuralgia) persists long after the eruption has subsided. Usually, scars remain, and they may be extremely deep. Autopsies have shown, beyond room for doubt, that in such cases the nerve trunk supplying the affected cutaneous area, and especially the ganglion upon its dorsal root, are the seat of inflammatory and degenerative processes. Unfortunately, with the means at our command the lesion has not been traced into the terminal filaments and end-organs of the affected nerve in the cutis and epidermis. The demonstration is, however, almost complete that we are here in presence of a neuro-cutaneous and continuous lesion, corresponding to the continuous lesion observed in (*a*) the neuro-muscular apparatus. Here again we have to deal with an evidently non-traumatic and non-infectious lesion (the skin lesion), revealed by regular and constant symptoms, unpreventable by mechanical means and incurable in a strict sense of the word.

Similar herpetic cutaneous lesions are observed after injuries, more especially such as give rise to irritative and inflammatory conditions of the nerves. Simple section is more apt to be followed only by cutaneous alterations of the first class. It is, furthermore, possible

that traumatic neuritis may produce other true lesions of the skin besides herpes, but this is not yet proven.

To these two varieties of lesions, embraced in the second of the classes which I propose, I am ready and willing to apply the term *trophic lesions* in the true sense of the word; *i. e.*, they are *histological alterations set up directly and fatally by the nerve disease, without the intervention of accidental or extraneous causes.* The relation of cause and effect seems indisputable, and we may therefore say that the efficient cause of the trophic lesion is disease of a part of the nervous system.

And, now, as to the mechanism or physiology of these trophic lesions. We cannot go far in this direction without entering the domain of pure speculation. It is only a few weeks since I heard my illustrious master and friend, Professor Charcot, state in a clinical lecture that we know absolutely nothing of trophic nerves and their mode of action. In this negation I most fully concur, especially if it be applied to the confused or unclassified mass of so-called trophic lesions about which so much has been written. The existence of trophic nerves as such, is unanimously denied by physiologists, and much of the speculations of physicians have been made without scientific basis in anatomy and physiology.

The attempt to simplify the problem which I herewith submit to the Association may not advance our actual knowledge, but it may possibly serve as a step toward a more exact study of the subject, and may give rise to a beneficial discussion. Allow me, in closing, to attempt to show in what way this analysis may be a slight step in advance.

I have, in the first place, rejected from the category of trophic lesions all vaso-motor, calorific, and metabolic phenomena, as well as all mere quantitative reductions in tissues and organs; reserving the name for such alterations as are characterized by demonstrable histological changes. This will doubtless be objected to, as in a certain sense the excluded phenomena have much to do with nutrition, and the word "trophic" leads the mind inevitably to think of *changes in nutrition*, which for me is far too vague and general a conception to prove of help in the study of our subject. Besides, since Claude Bernard's, Brown-Séquard's, and Ludwig's discoveries, a sort of antagonism has

been revealed between mere vaso-motor variations and the activity of the cellular life (salivary secretions, etc.).

In the second place, I have attempted to show that histological lesions, apparently due to nervous disease, may be divided into two classes: one in which the morbid nervous influence is of doubtful, or at least secondary causal value, while the active or efficient causes of the lesions are extraneous and accidental (traumatism and infection); whereas in the second class (by far the smaller at present) extraneous causes are unimportant, or even wholly wanting, while, as far as our present means of observation go, the efficient cause of the lesions is a morbid state of the nervous system.

The phenomena which make up the first class, I hold to be mere complications having a complex etiology, while those of the second class are really trophic lesions due to disease of the nervous system.

I would not be understood as claiming that the classification here proposed is final or absolutely exact in all its details. For example, lesions of the second class may co-exist with others of the first class in paralyzed parts: *e.g.*, atrophic or herpetiform lesions in paraplegia of the traumatic form especially. Again, a reasonable doubt may be entertained as to whether arthropathies always belong to the first class of lesions. It would be necessary to make a new analytical study of all varieties of so-called trophic lesions, and classify them according to their histology and etiology, in the light of the subdivision here proposed.

Third. While not pretending to be able to throw any new light on the intimate nature of real trophic lesions, I desire to point out that possibly (and I say this with all due reserve) the mechanism of these alterations, or, as the question puts it, the relation between the trophic lesions and disease of the nervous system, lies or is embraced in a law of *inter-dependent life in continuous tissues*. As regards the neuro-muscular changes of our second class, the operation of such a law seems highly probable. Anatomically and physiologically the neuro-muscular apparatus from the ganglion cells of the ventral cornua of the spinal cord to the striated muscular substance is a unity or becomes one before the completion of fœtal life. Whether the ultimate nerve fibrillæ and other prolongations of neural substance which lie under the sarcolemma actually blend with the sarcous substance, is, I know, an unsettled point; but their coaptation and physiological con-

tinuity are established. Besides, in lower animal forms true neuro-muscular structures do exist.

With reference to the neuro-cutaneous apparatus, we need still more delicate and reliable histological researches to show what is the true relation between ultimate nerve fibrillæ and the peripheral neural substance with the cells of the cutis and epidermis. While some observers claim actual blending of the two substances, by penetration of nerve fibrillæ into epithelia and by the interposition of cell-like nervous expansions in among the cells of the skin, others are in doubt as to the arrangement. Still, at the present time, the weight of evidence is in favor of the existence of continuity between sensory nerves and some of the elements of the skin. The same statement may be made with reference to the relation existing between the termination of glandular nerves and epithelia of glands.

I therefore venture to suggest that *disease of the nervous system produces true trophic lesions when it interferes with the associated or inter-dependent life of continuous tissues.*

THE RELATION BETWEEN TROPHIC LESIONS AND DISEASES OF THE NERVOUS SYSTEM.[1]

By H. C. WOOD, M.D., LL.D.,
PROFESSOR OF MATERIA MEDICA AND THERAPEUTICS, AND CLINICAL PROFESSOR OF DISEASES OF THE NERVOUS SYSTEM IN THE UNIVERSITY OF PENNSYLVANIA.

SPEAKING as the representative of the American Physiological Society, I wish to look at the physiological rather than the clinical side of the questions: "What proofs have we that the nervous system is capable of directly influencing the nutrition of distant portions of the body, and what evidence is there that any such power is exerted in disease?" For the sake of clearness, I shall arrange my remarks in support of general propositions.

The first of these propositions is: *It is physiologically proven that the nervous system directly affects general nutrition.* All histologists agree that there is a very close connection between the peripheral ends of the motor nerves and the sarcolemma of the muscles; and all physiologists recognize that when a nerve trunk is divided the tributary muscles undergo a peculiar and rapid change of structure, which is not the result of the alteration of the blood supply, and is not dependent upon any action of the nervous system on the bloodvessels. It would seem that these well-known facts are in themselves sufficient to demonstrate that the nervous system has some direct control over nutrition, but the function of the muscles is so distinct from that of the other tissues, and is so palpably influenced by the nervous system, that not only clinicians, but even physiologists, have been led to consider muscle as something different from other portions of the organism in its subordination to the nervous system. When, however, we come to study the relation between the nervous system and glands we find that

[1] This address was originally made almost entirely without notes, but was not reported at the time owing to the sudden illness of the official stenographer. This article is an abstract prepared by Dr. Wood, three months after the meeting, as embodying the substance of his remarks.

the anatomical and physiological evidences of a nervous control of the function and nutrition of the gland are just as positive and just as clear as those which we rely upon as proving that nerve does influence muscle.

The histologist has shown that there are certain nerve fibres which are anatomically connected with glands. Landois defines, in his systematic treatise on physiology, "secretory nerves to be those nerve fibres whose peripheral end-organ consists of a secretory cell, the central ends of the fibres being connected with nerve cells." Pflüger affirms that the axial cylinder of the secretory nerve terminates in the nucleus of the secretory cell: Landois also teaches the direct termination of the nerve fibres in the secretory cells of the salivary glands. Physiologists have shown that stimulation of the chorda tympani nerve produces a copious secretion of watery saliva, accompanied by vascular dilatation in the gland, and a consequent increased flow of blood; while stimulation of the sympathetic nerve causes contraction of the bloodvessels, with the secretion of a thick, scanty fluid rich in the peculiar organic constituents of saliva. That the changes in the glandular circulation which are produced by the stimulation of the nerves are not the cause of alterations in the secretions, is demonstrated by the well-known facts: first, that after ligature of the bloodvessels of the gland, and even after decapitation, stimulation of the chorda tympani still causes an increase of the salivary secretion: second, that certain drugs have the power of affecting one function of the nerve without influencing the others, thus, after the injection of atropine, stimulation of the chorda tympani still causes dilatation of the vessels, but does not affect the secretion. Without occupying further time with the detailed statement of what I may be allowed to call textbook physiology, I only desire to call attention further to the important fact that stimulation of the chorda tympani nerve causes positive variation of the electrical condition of the salivary gland, whilst stimulation of the sympathetic nerve invokes a negative variation, so that there is evident antagonism between the two nerves.

The facts of the case are so strong, and have been so clearly established by physiologists, that I think that clinicians must recognize that the protoplasm of gland cells is as closely and completely subordinate to the nervous system as is the sarcolemma of muscle.

Passing by the physiological facts whose truths have been sufficiently

long known to find their way into handbooks, we come to the very remarkable researches of Dr. W. H. Gaskell, which have been reported in the *Journal of Physiology*. It has been generally believed that the action of the vagus in inhibiting cardiac movements consists in an interference with the nervous apparatus of the heart, or, in other words, that cardiac inhibition is due to one set of nerves so controlling the ganglia in the heart as to prevent the nervous discharge which ordinarily causes contraction of the cardiac wall; and that in this interplay of nervous apparatus the muscular tissue is passive—is, as it were, only an interested bystander.

In the light shed by the researches of Dr. Gaskell and his followers, this explanation, however, appears to be incorrect. It has been shown that a weak interrupted current passed through a strip of insulated muscular tissue taken from the frog's heart, produces upon this tissue all the effects of inhibition; and that atropine prevents such inhibitive action precisely as it prevents the action of the vagus upon the uninjured heart; further, that if a strip of suspended heart muscle be firmly fastened in the middle by a clamp, it is possible to produce inhibition of the lower segment by stimulating with the weak interrupted current the part of the muscle above the clamp. Of the latter phenomenon two possible explanations offer themselves at once: first, that the inhibition of the lower segment of muscle is due to the excursion of the electrical current from the upper to the lower portion of the muscles; secondly, that the upper portion of the muscle contains minute nerve fibres of inhibition passing down to the lower muscle mass. The correctness of the latter of these two explanations seems to be proven by the fact that the tightening of the clamp will prevent inhibition of the lower segment by the irritation of the upper portion of the muscle, since tightening of a clamp would arrest the functional activity of nerve fibres, but would not interfere with the downward propagation of an electrical current. Again, it was found by Dr. Gaskell, in the crocodile, that if the vagi be divided, and time allowed for the degeneration of the peripheral ends, the effect of the weak interrupted current in inhibiting the strip of isolated muscle was in great part or altogether lost.

This research of Dr. Gaskell has received confirmation in the work of Mr. T. Wesley Mills, of Montreal, upon the heart of a fish, who found that stimulation of the auricle leads to an immediate arrest of

movement, and that atropine directly applied to the muscle prevents inhibitory arrest precisely as it puts aside in the mammal the ordinary effects of stimulation of the vagus. Dr. J. A. McWilliam found in the eel's heart, when the inhibitory nerves were fully excited, that the strongest mechanical, chemical, and electrical irritations of the heart muscle were powerless to produce contraction: showing that in the eel the inhibitory nerve, when excited, so directly and profoundly influences the cardiac muscular tissue as to deprive it entirely of its functional power. Such action as this must be exerted directly upon the muscle-tissue. Dr. W. B. Ransom has proven that in the cephalopods the rhythmical contracting power resides in the muscular fibres of the heart themselves, and that the irritation of the so-called visceral nerves, which evidently correspond to the vagus of the mammal, inhibits the cardiac muscle by acting directly upon it.

These various facts demonstrate that the inhibitory nerves in the heart act directly upon the muscle; and it has been proven by Dr. Gaskell, that during the stimulation of the vagi nerve, although there is complete arrest of the functional activity in the muscle, there is a positive variation in the electrical condition; whilst any nervous influence which throws the heart into activity produces a negative variation. It has also been shown by Gaskell that the inhibitory process is beneficial to the cardiac muscle, and that during it damage to the tissue is repaired, and loss of power is regained. It would appear, therefore, that precisely as is the gland cell, so also is the cardiac muscle dominated by two sets of antagonistic nerves: *katabolic nerves*, which produce, when excited, the phenomenon of functional activity with an after-exhaustion of the part; *anabolic nerves*, whose excitement causes a lessening of functional activity, followed by a regaining of power—katabolic nerves, the nerves of functional activity and destruction: anabolic nerves, the nerves of rest and nutritive repair.

In a research which I made some years ago, in regard to the thermic function of the body, certain facts were proven which indicate that what is true of muscle and gland is also true of other portions of the body. It has been long known that galvanism of a sensitive nerve produces marked disturbance of the circulation and a fall of the bodily temperature; and the general belief has been that the first of these phenomena is the cause of the second. I found, however, that the rise of the arterial pressure occurs immediately after the first irritation of

the nerve; while the fall of the bodily temperature only reaches its maximum long after the cessation of the irritation, and continues for many minutes. Thus, in one experiment, the temperature-fall, which amounted to $3\frac{3}{4}°$ F., did not reach its maximum until twenty-three minutes after the last irritation of the sensitive nerve, a time when the disturbance of the circulation had long since ceased. I, therefore, reached the conclusion that the fall of temperature which resulted from the irritation of the sensitive nerve is entirely independent of the circulation: a proposition whose correctness was demonstrated by finding that when the medulla had been separated from the pons at the point of junction, irritation of a sensitive nerve produced the ordinary disturbance of the blood pressure, but was unable at all to reduce the temperature, which steadily mounted upward. As the fall of temperature which is produced by irritation of a sensitive nerve is independent of the circulation, and as it cannot be caused after separation of the medulla from the pons, it seems to be an inevitable conclusion that when it occurs it must be the result of an action upon a nerve centre situated either in or above the pons.

I also proved that separation of the medulla from the pons is followed by a great rise of animal heat, and that this rise of animal heat is not accompanied by any perceptible change either in the respiration or circulation—the centres of respiration and circulation both being below the point at which the medulla was divided. The rise of temperature was proven not to be due to the retention of animal heat, but to be caused by an enormous increase in the development of heat.

These facts are, apparently, susceptible of but one explanation, namely, that there is above the medulla a nerve centre which, when stimulated, controls or inhibits the production of animal heat—*i. e.*, tissue change—and whose separation by section from the tissues is followed by an enormous increase of the production of animal heat; such a centre must evidently be inhibitory in its action, and have the same relation to the animal heat-production that the vagus has to the cardiac movement. It is a great anabolic centre.

In *résumé*, I may add that, in the light of these various researches, it seems to me that we are warranted in concluding that there are within the body two sets of nerves, katabolic and anabolic.

In the ordinary nerves of the extremities, we have at present proof of the existence of only one class of efferent fibres, namely, the so-

called motor fibres—those which excite the function of the muscle; these are the katabolic nerves of the voluntary muscles, but I believe that anabolic fibres will at some future time be found to exist in these mixed nerve trunks.

We have at present, however, knowledge of two sets of nerves supplying the muscular fibres in the walls of the bloodvessels: one whose excitation produces contraction, one whose excitation produces relaxation and consequent dilatation of the vessels: the first of these, the so-called vaso-motor nerves, are true katabolic nerves; the second, the so-called vaso-dilator nerves, true anabolic nerves.

So far have we progressed in this discussion, that I think we are now prepared to answer the first of the questions asked in the beginning of my talk. I believe we must consider it definitely proven that the nervous system has the power of influencing nutrition; indeed, I will go further and affirm that the only function which the nervous system possesses is the function of influencing nutrition. The peripheral afferent nerve-ending discharges its force because its nutrition is affected by the external irritant; this nerve force travels up the sensory nerve by producing an alteration in its nutrition and the motor centre gives back the replying discharge of force because its nutrition is disturbed; whilst, finally, the outgoing nerve and the muscle suffer in their nutrition and become thereby functionally active. Functional activity and exhaustion, functional rest and repair of the nerve, of the muscle, of the gland—all of them nutritive movements, each of them dominated by the nervous system. Such, in a sentence, is the ground-plan of the animal organism.

The second proposition which I shall bring forward is: *Various lesions are the immediate result of previous nerve disease or nerve injury.* In supporting this proposition, I shall not attempt to discuss the large numbers of lesions of muscles, fibrous tissues, and bone which are well known to occur in various chronic diseases of the nervous system, but shall confine my attention to those lesions of structure which follow so certainly and so immediately upon sudden disease or upon injury of nerves or nerve centres that there can be no question as to the causal relation between the nervous trouble and the so-called trophic change.

Of all the lesions which are apparently produced through the ner-

vous system, the most satisfactory for our present purpose, and the most undeniable in their relations, are those which are consequent upon injury of the nerve. In 1874 Kraske (Halle, Theses) noticed as the result of cutting the maxillary nerve of young rabbits, ulcerations of the lip similar to those subsequently reported by Wilhelm Stood, who in 1881 repeated and extended the experiments of Kraske. In rabbits of from six to twelve weeks old, four days after incision of the medullary nerve there appeared upon the inner surface of the affected lip a small yellowish ulcer which rapidly grew, but upon the fourteenth day had begun to heal. In other rabbits the irritation of the nerve produced intense inflammation of the lip with ulceration in a position similar to that just described.

Max Joseph, in 1885 (Virchow's *Archiv*, vol. cvii. p. 127), extirpated two of the right cervical nerves. Seven days after the operation there appeared a circumscribed spot upon the surface with regular boundaries in which the hair was much thinned. This spot rapidly increased in size, and was shortly added to by the coalescence with it of two similar smaller spots; then the hair became thinner and thinner until there was absolute baldness. In one case Joseph cut the cervical nerves between the spinal cord and the spinal ganglia on the right side, and on the left cut the nerve distally to the ganglia. The result in the right side was no change in the nutrition of the hair; on the left side alteration in the usual manner. Very careful examination of the bald spot showed complete absence of vascular dilatation, general inflammation, and microscopic organisms; and the purest picture of atrophy of the hair was revealed, the hair papilla becoming atrophic and the hair dropping out.

In 1876 (*Gaz. Méd. de Paris*, vol. xlvii. p. 255), M. Couty cut the posterior roots of the nerves going to the anterior feet of a frog. Five days after the section the knee-articulation was not only anæsthetic, but also somewhat swollen, and the whole leg was œdematous. The arthritis and the œdema both increased and when the frog died, five weeks later, the extremities of the tibia and the femur were swollen and red, and at least double the size of the corresponding side, whilst the synovial membrane was infiltrated, thickened, and more vascular than normal.

The lesions of the eye which follow section of the trigeminus in the lower animals are too well known to require description here. It is

so generally believed that they are not the direct result of the injury, but are due to the withdrawal of the influence of the normal excessive sensibility of the eye that I shall not enlarge upon them, calling attention only to the facts that Schiff affirms that he has partially cut the twigs of the trigeminus in such a way that sensibility has been preserved and no inflammation of the eye has occurred, and that Meissner also reports three cases in which the trigeminus was partly cut and in which there were no trophic disturbances of the eye, although sensibility was lost.

The results of nerve injuries in the human being are even more decisive as evidence that the nervous system is capable of producing alterations in the nutrition of tissues than are the experiments made upon animals. As long ago as 1859, Charcot recorded two cases in which vesicular or bullous eruptions followed traumatisms of the nerves of the arm. In 1862 Reynaud recorded a case in which a similar eruption was the result of an injury to the cubital nerve, and was absolutely confined to the territory supplied by that nerve. Earl has recorded one in which the eruption was the result of an injury to the external cutaneous nerve; Denmark, one in which the vesicular eruption was the result of injury to the radial nerve; Kuhl, one in which the injury of the brachial nerve was followed by herpetic eruption. I myself have seen cases of exactly this character—that is, cases in which traumatism of a nerve was followed in one or two days by an eruption of vesicles confined in location to the distribution of the injured nerve. Every clinician knows that the herpes zoster is usually connected with, and apparently dependent on, idiopathic or gouty neuritis.

It is hardly necessary in this assembly to do more than recall the thought to the large number of cases of so-called glossy skin reported by our late president, Dr. S. Weir Mitchell, during the War of the Rebellion—cases in which the changes in the skin followed directly upon a traumatic inflammation of the nerve; cases which have since been duplicated in the practice of most neurologists.

It is well known that in acute inflammation of the spinal cord rapid sloughing of the skin and subjacent tissues, involving even the bony structures, is liable to occur, and to constitute the condition known by the French as decubitus. This spinal bed-sore may be produced in the lower animals as well as in man. Brown-Séquard states, in the *British Medical Journal*, 1880, vol. ii. p. 915, that he has seen local

death commence on the sacrum of a dog seven hours after the inflammation of the spinal cord had been excited; and that within twenty-four hours the skin became violet, and completely gangrenous. Some pathologists teach that this destruction of tissue is not the result of any direct action of the nervous system upon the part, but it is due to pressure, or irritation by the urine, feces, etc., and that it may be prevented by great care in nursing. Undoubtedly, pressure and the irritation of foul discharges may hasten the sloughing of the buttocks of the man suffering from myelitis, but just as certainly, in many cases, no amount of care will prevent the formation of rapid bed-sore. In the experiments of Brown-Séquard just spoken of, there was neither pressure nor urinary or other irritation to cause the sloughing off of the soft parts from the sacrum of the unfortunate dog; and I have seen the buttocks of a man melt down when by the use of the water-bed and other means everything that could be had been done to prevent the decubitus.

Further, I must insist that if a buttock is in such a condition that a little pressure due to lying upon it, or a little soiling by the natural discharges, will cause it to slough away, such buttock is in a most unnatural state. It is the function of the part to be sat upon, and to be irritated without suffering. There is, certainly, as the result of an acute spinal inflammation, some strange alteration in the nutritive condition of the region of the gluteal muscles which strongly predisposes the part to death.

Without occupying more time with the matter, I think I have brought forward enough of evidence to demonstrate my second proposition, and to prove that certain diseases and injuries of the nervous system are capable of producing profound structural changes not only of the muscles, but also in the other soft tissues of the body.

The third proposition which I desire here to support is: *A distant lesion may follow a nerve injury or a nerve disease without any precedent disturbance of the local circulation.* In many cases it is very difficult or impossible to determine the absence or presence of a local alteration of the circulation preceding the distinct trophic change: such is not true of decubitus. Many years ago Brown-Séquard showed that hemisection of the spinal cord is followed by a loss of motility and vaso-motor paralysis on the side of the section, and that on the side opposite the section there is simply a loss of sensi-

bility without disturbance of the power of motion or the normal temperature. Since the publication of the researches of Brown-Séquard, cases have been published by Müller, by Brown-Séquard, by Jolffroy, by Charcot, and others, in which, after hemisection by accident or disease of the spinal cord, decubitus has occurred, not upon the side on which the muscles and the vessels have been paralyzed, but upon the side on which there has been no visible alteration of the bloodvessels, and no rise or fall of the local temperature. Charcot affirms that the disturbance of the nutrition is *always* on the side on which sensibility alone is paralyzed; and believes that although the centres which dominate the nutrition of the muscles are in the anterior part of the gray matter of the cord, and send out their fibres in the so-called motor nerves, that the trophic nerves of the skin and cellular tissue are in the posterior portions of the cord, and are connected with the sensory nerves.

My fourth proposition is: *Alterations in the condition of the vaso-motor centre are not capable of causing many of the distant lesions which follow injury or disease of the nervous system.* Increased flux of blood to the part, so far as our present knowledge goes, has no very pronounced effect upon the nutrition of the part, or at most, if very acute, only produces some general inflammation, or, if chronic, some slight hypertrophy. An irritant which has no specific effect simply increases the supply of blood to the part, and causes a general, widespread hyperæmia, followed, if the application be prolonged, by a general, not a peculiar, inflammation. It is almost impossible to conceive that the simple paralysis of the vessels of the skin and subjacent tissue should manifest itself by such a peculiar local and partial change in the structure as is involved in the formation of a vesicle. It is conceivable that shutting off the supply of blood to a part might produce the sloughing of a decubitus, but the continuance of the white waxy fingers of Reynaud's disease shows that the almost absolute spasm of the bloodvessels may continue for many hours and days without causing the death of the part supplied.

We are not, however, left to such general considerations as these just given; we have abundance of direct evidence to show that a simple vaso-motor palsy, although causing violent congestion and pronounced rise of temperature, produces no alterations in the nutrition of the part. It is well established that after destruction of the cervical sympathetic

ganglia of the rabbit, although intense hyperæmia of the ear may persist for months and years, the ear undergoes no alterations in its structure save only in so far as some observers believe that the hair becomes a little more abundant. It has indeed been asserted that in such an ear inflammation is more easily produced than in the normal rabbit, and is more violent and uncontrollable in its course; but Bernard found that so long as his animals were well fed and healthy the vaso-motor paralysis made no difference in the condition of the ear; whilst Pye Smith, of London, has kept rabbits with divided sympathetic nerves for over five years, and has noted that the ordinary exigencies of rabbit life excited no inflammation and no perceptible change in the ear whose vessels were completely paralyzed.

I believe that I have demonstrated that the nervous system directly affects the nutrition of tissues, that various organic alterations are the result of previous nerve injury or disease, that these so-called trophic lesions are not necessarily preceded by local circulatory disturbances, and that there is no known vaso-motor disturbance which is able to produce such lesions. Under such circumstances it seems to be absurd to attribute change in distant organs, consequent to nerve injury or nerve disease, to local disturbances of the circulation; or to claim that they are other than the direct outcome of the nervous disorder. Up to almost the present time, those physiologists and clinicians who believed in the theory of trophic change, have usually or always been upon the defensive; but accumulating knowledge has entirely changed the relative positions of the two sets of theorizers, and I must insist that the time has come when the deniers of the trophic theories must assume the defensive. At present, whenever any distant lesion is known to follow a disease or an injury of the nerve or nerve centre, the burden of proof rests not upon him who asserts that such lesion is the result of a direct nervous influence, but upon him who declares that it is an accidental occurrence or is caused by the alteration in the supply of blood to the affected part.

DISCUSSION.

DR. W. M. ORD, of London: I feel very much honored at being asked to speak on this important subject, but before I say a word I beg to tender the Association my gratification at the very high tone and character of the discussions on the papers I have previously heard in this room.

As regards the two papers I have heard read, they have certainly stood in strong contrast with one another: the one critical, the other appealing strongly to the imagination and fascinating one by its strong and logical character of thought and imagery. I will try to prove from the start how from disturbances of nutrition there is a starting-point in the nervous system of the local trophic disturbances. I do not think Dr. Seguin would deny that the changes in chronic rheumatism are of a histological character. I will quote two cases bearing upon this point which I saw recently before leaving London, having them under observation for more than a year. The first was a case of chronic hypertrophic cervical pachymeningitis, which presented a very typical form of that disease. This patient (a young man) at the time of his admission showed very little change in the skin and none at the joints. He was kept in bed, treated, and recovered to a large extent his motor power, and completely his sensation. There was a steady wasting of the tissue around the second joint and third phalanges of the digits, a distinct and marked change in the joints. The joints of the thumbs and fingers became thickened, somewhat tender, and finally loose. All the physical signs of progressive chronic arthritis became developed in conjunction with dystrophy of the skin and muscle without any local irritating cause. The next case was an old man, who for many years lost power and underwent atrophy of the muscles in the upper extremities, as the result of an affection of the anterior column of the spinal cord. This man had a wasting of the skin and a very remarkable change in the joints. They were considerably swollen, were constantly dislocated, and as constantly put back in place. The whole muscles of the upper extremities were wasted. He sat up in bed with great difficulty. These two cases illustrate very well the primary affection of the nervous system in determining dystrophy of the joints.

Early in my endeavors to analyze osteo-arthritis I was brought face to face with a class of cases which interested me very much. As a woman approaches the climacteric period one sees a painful swelling of the small joints of the fingers and toes—nodosity of the joints (Heberden). Examining such cases with great care, the one thing always present was disorder of the catamenial function, and chiefly such a disorder as consisted in menorrhagia. I have seen cases in which, as the climacteric began to develop, the joint affections returned with intensified severity; but the day before I left London I saw two cases: one was a lady of sixty years, who had troubles of this kind during the climacteric period, and had a large uterine fibroid. Here was an unusually persistent type of subacute form of this disease long after the cessation of the catamenia. People who suffer in this way are, in all cases, very excitable, anæmic, and sensitive women. It appears to me that, as affections of the nerves or spinal cord may bring about neurotic dystrophies in joints, so local irritation may produce, in a reflex way, affections of joints more or less parallel to the affection determined by nerve disease. In the same way I have great reason to believe that continued prostatic irritation will produce affection in the joints of the leg. Gonorrhœal rheumatism may itself not be the result of a microbe, but the work of the nervous system.

Acute rheumatism might be traceable to similar causes. It seems to me that acute rheumatism is often developed under conditions which would indicate affections of the nervous system, such affections that would bring about, in a reflex way, the irritation of the joint. It occurs certainly after parturition. It occurs almost invariably in people who are very anæmic, or have lost large quantities of blood, and whose nervous system is brought into a state of over-responsiveness.

DR. M. ALLEN STARR: I have very little to add to the discussion. It seems to me that the paper of Dr. Seguin marks a decided advance toward a certain amount of clearness in the study of trophic disturbances. The point he makes that trophic disturbances must be distinctly separated from nutritive disturbances is one that has not been appreciated, and it seems to me to be a very important one, and throws considerable light on the subject.

There is a class of nutritive disturbances that has not been mentioned yet, which has been described by Dr. Osler as infantile cerebral palsies, viz., that class of atrophies which follow hemiplegia in children. In adults hemiplegia does not appear to be followed by any amount of atrophy on the paralyzed side. In children this is not so, and Dr. Seguin's idea of nutritive disturbances occurring in a physiological system seems to throw light on this atrophy. It is important to separate vaso-motor from trophic disturbances. Trophic disturbances follow very closely on vaso-motor disturbances, and it is not strange that the two should be confounded.

DR. H. P. BOWDITCH, of Boston: I agree entirely with Dr. Seguin that any attempt to explain the mechanism of trophic changes must be speculative in our present knowledge. We have to recognize in muscular tissues two distinct metabolic processes, one having its cause in the nitrogenous and the other in the non-nitrogenous metabolism. These are distinct, inasmuch as it has been shown that the nitrogenous metabolism is not connected with muscular activity, while the non-nitrogenous is. When the muscular activity is kept within physiological limits urea excretion remains the same, but nitrogenous metabolism is, nevertheless, essential for muscular activity. It is essential for muscular activity that an abundant stream of nitrogen should flow into the body and out again, in order that the muscles may be kept well nourished and capable of work. So the nitrogenous metabolism of the muscles goes on quite independently of its functional activity. If we limit the term trophic to the nitrogenous metabolism of the muscle and regard the non-nitrogenous metabolism as associated with the functional activity, we would simplify matters a good deal.

DR. PYE SMITH, of London: I have listened with great pleasure to the two admirable papers read before the meeting, but confess from a physiological and clinical standpoint my judgment would be on the side taken by Dr. Wood with such eloquence and zeal. I do not, however, agree with Dr. Wood in imputing a logical error to those who resort to the supposition of injury or of vaso-motor disturbance to explain the phenomenon in question.

I would quote the case reported by Hilton, of a patient with an ulcer in the hand, which got worse and worse in spite of all endeavors to cure it, until it was discovered that a minute exostosis was pressing on the ulnar nerve, and when the exostosis was removed the ulcer healed rapidly. Gangrene, caused by an embolus obstructing an artery, is a well-known fact; but there are cases of gangrene inexplicable on this hypothesis. We have all seen strange eruptions come on rapidly after lesions of the cord; and my own experience is that these trophic cutaneous lesions are bullous or herpetic in character. I think from these and other facts adduced by the speaker, that trophic nerves have a true position, not only in physiology, but in medicine.

Dr. David Ferrier, of London: I am of the opinion that there can be no question of the direct influence of the nervous system on the nutrition of the tissues altogether apart from vaso-motor changes and agencies operating *ab-extra*. This is shown, among other things, by the structural modifications which are observed in the glands on stimulation of their secretory nerves; and by the degeneration which ensues in muscles which are separated from the cells of the anterior cornua, or when these latter are destroyed.

This degeneration I hold to be the direct result of severance of the muscles from their trophic centres, and is not due to secondary inflammatory changes, inasmuch as it occurs under conditions which entirely obviate the initiation and propagation of irritation. But I believe, with Dr. Seguin, that the great majority of the trophic disturbances which so commonly occur in connection with injuries of the nerves, such as panophthalmitis from lesion of the fifth, and the herpetic and other cutaneous disorders from lesion of common sensory nerves, are not due to the withdrawal of trophic influence, but are the results of secondary inflammation or injurious agents operating from without. They are observed more especially in connection with partial lesions of nerves and those most calculated to excite irritation.

I should, however, expect to find (although I am not aware that the subject has been fully investigated) that section of all the sensory nerves of any given region would induce degeneration of the nerve-endings and their annexes, precisely analogous in character to the degeneration which occurs in muscles when the motor nerves are divided, or the anterior cornua destroyed.

But the point is whether these and similar facts are to be regarded as proofs of the existence of a special system of trophic nerves and trophic nerve centres, distinct from those which minister to the general functions of motion (secretion) and sensation, or whether the maintenance of nutrition of the tissues is merely a different aspect of the general functional activity of sensory and motor nerves.

I have, on the principle "entia non sub multiplicanda," been more inclined to adopt the latter view, and to regard, with Sigmund Mayer and Dr. Seguin, the muscles, nerves, and anterior cornua as forming together a functional and nutritive unity. In combination these structures exhibit what we call normal laws of reaction and nutrition; but when separated they are reduced each to its own independent vitality, and exhibit the changes which we

are accustomed to call those of degeneration, characterized by the retrogression of the more highly organized, and proliferation of the less highly organized structures.

The recent important researches of Gaskell have thrown new light on the structure and functions of the nervous system, more especially the nerves which innervate the walls of the bloodvessels and the hollow viscera.

There are two sets of nerves possessing diametrically opposite functions. One set (*katabolic*) excite activity and cause waste; the other set (*anabolic*) inhibit activity and promote repair. It may be that the inhibitory or anabolic influence is not, like the katabolic or motor, the direct result of nerve stimulation, but only a question of interference of nerves alike in function. But if the facts cannot be so explained, then there would appear to be (in the case of the muscles of organic life at least) a system of nerves which would be properly called trophic.

Whether this differentiation of nerves obtains also among the muscles of animal life is a point which has not been, as yet, determined.

I am of the opinion that more light will be thrown on the subject by the researches on thermal or heat centres now being actively pursued, and on which Dr. H. C. Wood and Dr. Ott have given us valuable contributions.

The question is whether there are nerve centres which influence and regulate the production of animal heat, apart from those which are related to motion and sensation. Many facts have been induced in favor of this proposition, but, I think, much will have to be done before we can regard it as proved.

The muscles are the chief foci of heat-production in the body. I should, therefore, argue that the centres of heat-production must be coextensive with those which minister to the nutrition of the muscles. I am, therefore, not readily inclined to believe that there can exist, anywhere in the nervous system, any one centre capable of maintaining (as a nerve centre must) afferent and efferent relations with the whole muscular system. So far, as yet appears from experimental research, increased production of heat has been found in association with initiative lesions of nerve centres and tracts; but I am not aware that it has been proved satisfactorily that increased heat-production and increased tissue-waste result from the removal of a centre whose functions would, with propriety, be termed inhibitory or anabolic. But I think that researches in this direction are those which are best calculated to throw light on the difficult problem at present under discussion.

MR. VICTOR HORSLEY, of London: I beg to be allowed to communicate the results of experiments performed by Dr. Frederick Mott, of London, in the laboratory of the Brown Institution, since it seems to me that Dr. Mott's observations are of the utmost importance, especially in view of the direction which the present discussion has taken.

The question appears to me to present the following points. Destruction of what we regard as trophic centres, or cutting of fibres leading from the same, may presumably produce disturbances of nutrition in one of three ways or combination of these, viz.:

1. True trophic disturbance.
2. Loss of function.
3. Vaso-motor changes.

Vaso-motor changes, I think, are anyhow put out of the question, by the fact that experimental removal of the superior cervical ganglion of the sympathetic produces practically no change. Dr. Mott's method of experimentation appears to me to possess the especial merit of excluding not only this complication, but also that due to loss of function. His method was as follows:

He ligatured, on one side, certain anterior roots in the cauda equina of monkeys, under strict antiseptic precautions, and then observed the condition of the femora on the two sides. Now the normal metabolic changes in bone are, of course, known since the days of Hunter to be a combination of absorption and restoration. Dr. Mott found that the trophic change in the bones produced were destructive—*i. e.*, exaggeration of the normal process of absorption. The femur of the side on which the roots were ligatured was probably atrophied, and, on microscopic investigation, Dr. Mott discovered that the medullary surface of the interior of the bone was thickly covered with osteoclasts. Considering that the function of the bony skeleton is so essentially passive, especially in contradistinction to the example of muscle quoted by previous speakers, and considering the small influence which vaso-motor changes could be imagined to bear on the nutrition of such a resistent tissue as bone, I think that Dr. Mott's experiments definitely supplied proof for the first time of katabolic changes, or exaggeration of normal life processes, originated by lesion of efferent nerve fibres.

Dr. Seguin: I have been very much pleased to find that my subdivision of these lesions, and my conclusions, have not met with more contradiction. I am glad to learn that, without any previous consultation, Professor Wood and myself have advanced a strictly solidistic view of the relation of trophic lesions to diseases of the nervous system, rejecting vaso-motor factors. This is in the line of his own analysis.

With reference to Dr. Ord's interesting statement about joint lesions of remote origin, I would say that there are seemingly strong arguments in favor of ranking arthropathies in the list of true trophic lesions, and that possibly in this respect the proposed classification might need amending. Yet, I would call Dr. Ord's attention to the fact that it is extremely difficult, if not impossible, to exclude trauma as an exciting cause in cases of trophic (and other) arthropathies. The reason why joint lesions are seldom observed in cases of partial paraplegia from myelitis, etc., and are common in tabetic patients is, it seems to me, that the latter are rash and reckless in their attempts to get about, and that having preserved muscular strength with loss of sensibility to pain, they are especially apt to injure themselves. Patients with paretic limbs, on the other hand, are usually cautious, and too weak to do themselves harm. Lastly, I would again insist upon the great rarity of arthropathies in patients whose circumstances enable them to take good care of themselves and avoid strain.

EFFECT OF SPINAL CONCUSSION ON THE REFLEXES.

By F. T. MILES, M.D.,

PROFESSOR OF PHYSIOLOGY AND CLINICAL PROFESSOR OF DISEASES OF THE NERVOUS SYSTEM IN THE UNIVERSITY OF MARYLAND, BALTIMORE.

One of the commonest symptoms in those injuries of the spinal cord which interfere with the transmission of voluntary impulses is the exaggeration of the spinal reflexes. I wish in this paper to call attention to some lesions of the cord of traumatic origin which, while cutting off the influence of the will, destroy the reflex actions of the parts paralyzed. The lesions I refer to are the result of, or accompany, jar or concussion of the spine.

As far as I know, very few observations on the loss of the reflexes in cases of concussion of the spine have been made, nor is the fact mentioned in our best text-books on diseases of the nervous system. In the numerous cases of injury to the spine by gunshot wounds recorded in *The Medical and Surgical History of the War of the Rebellion* this symptom is not mentioned. Liddell, in an article on "Concussions of the Brain and of the Spinal Cord," in *The American Journal of the Medical Sciences*, 1883, records the case of a man who received a blow on the back from the branches of a falling tree, in which there was motor and sensory paralysis of the lower limbs. He says "reflex motor action in the lower extremities was entirely suspended; for on tickling the soles of his feet and pulling the hairs of his legs, thighs, and groins, I failed to excite any motor reflex." Nothing is said as to the tendon reflexes, and he attributed the loss to concussion of the cord. An autopsy revealed injury to the cord in the cervical region. In the same article, Liddell quotes a case reported by Mr. Savory, in vol. v. of the *St. Bartholomew's Hospital Reports*, of a man who fell on his head, causing paralysis of motion and sensation of all four extremities, in which "no reflex action could be excited in the lower extremities, nor elsewhere." Autopsy revealed

a clot of blood in the cord in the cervical region. Hammond, in his *Diseases of the Nervous System*, under the head of "Spinal Congestion," and as an example of it, gives the case of a man "violently thrown to the floor of a railway-car in consequence of a collision with a stationary train in front," in which, "three months after," "reflex excitability was entirely lost."

The interest that centres around "concussion of the spine," and the importance of distinguishing its symptoms, with a view to correct diagnosis, as well as their physiological aspect, make me believe that the following cases, though imperfect in their history, are worthy of attention, and are suggestive of further investigation:

CASE I.—J. A., a man in middle life, brought to the Maryland University Hospital, with the history of having fallen from a window upon his back. When first received in the hospital ward he was examined by the surgeon in charge, who discovered no fracture or dislocation of the spine, nor indeed any traumatic lesion. The patient was then placed under my care. I found him completely paralyzed below the waist, both as to sensation and motion. The sphincter ani and bladder were involved, the urine and feces passing without consciousness on the part of the patient. Bedsores appeared, and even moderate pressure, as, for instance, resting one foot upon the other, produced blebs filled with bloody serum. Mind clear. Reflexes, both skin and tendon (using the latter term simply as expressive of a fact, not a theory), were absolutely lost. There was, moreover, no reply of the muscles to electrical stimulation. I gave a diagnosis of extensive softening of the cord, as it seemed to me that destruction of the gray matter of the cord could alone cause such abolition of the reflexes and such trophic disturbance.

I was, unfortunately, not present at the autopsy, and only the lower portion of the cord to a point above the lumbar enlargement was removed. There was no fracture or dislocation of the spine at the part opened, nor inflammation of the membranes. The portion of the cord removed was firm to the touch, and macroscopically presented nothing abnormal. It was immediately placed in alcohol, and afterward in bichromate solution. After hardening, it was microscopically examined, but nothing that could be certainly called abnormal discovered.

It may be said that if the cord had been examined in its entire length some point of lesion might have been found. Very likely some lesion might have existed higher up than the part examined, which would have accounted for the paralysis of sensation and motion, but how could such lesion explain the loss of the reflexes and the absence of electric excitability, and, we may add, the abnormal condition of nutrition? The reflexes, superficial and deep, depend upon the integ-

rity of the segment of the cord from which the sensory and motor nerves arise—in this case the lumbar enlargement, and here no gross lesion was discovered. A molecular lesion could only have been produced by the shock or jar the cord suffered.

CASE II.—D. E., man, aged thirty-three years. Had fallen from a scaffold thirty feet high. Admitted to hospital the next day, July 5, 1888. Fracture of the spine in the dorsal region, injury of the cranium and probably of the brain. Extension was used to reduce a hump in the back, apparently caused by displaced vertebræ. There were paraplegia, loss of sensation in lower limbs, paralysis of sphincters, absolute loss of the reflexes, superficial and deep, and no reply of the muscles to either electric current. Died August 7th, more than a month from occurrence of accident. No autopsy could be obtained.

CASE III.—F. A., man, aged twenty-two years, fell from a tree about twenty feet, on his back. This happened June 20, 1888, and he was admitted to hospital July 1st. No deformity of spine nor any outward indication of the injury on back. There were complete motor paralysis and anæsthesia below the nipple line. Sphincters paralyzed. Mind perfectly clear; complained of no pain; no discomfort. Temperature of lower limbs natural. Paralyzed muscles very flabby. A very little myotatic irritability. All reflexes—plantar, abdominal, cremasteric, patellar, Achilles tendon—entirely lost. No priapism. Neither electric current elicited any contraction of muscles. Died August 3d, more than a month after the accident. No autopsy was permitted. Here there was probably a local gross lesion of the cord, as there was a zone of hyperæsthesia above the line of anæsthesia.

CASE IV.—M. A., man, aged twenty years. Dived from a platform into water two feet deep, driving his head into the sandy bottom of the stream. When pulled out of the water, which was done immediately, he was perfectly conscious, but all below the neck was paralyzed to sensation and motion except some of the muscles of the left arm. His clothes having been immediately stripped off, the penis was found to be in a condition of extreme erection. A condition of partial erection continued until his death, five days after the injury, but the passage of a catheter did not increase it. Muscles not tested by electricity. Reflexes entirely lost. No autopsy,

Here, doubtless, there was fracture or dislocation of the lower cervical vertebræ, with destruction of the vitality of the cord at that point, the conditions not greatly varying from those in an animal that has been pithed, except for the jar or concussion which accompanied the injury, and to this I think we must attribute the sudden and persistent loss of the reflexes.

CASE V.—A. G., woman, admitted into hospital May 2, 1884, wounded in the left side of the neck by a conical pistol-bullet. Admitted just after the

injury. No wound of exit. Paralysis of the left arm, trunk, and lower extremities; anæsthesia below the level of first rib on the left, below the level of third rib on the right side. Sphincters paralyzed. Mind clear; complained of burning below the waist. All reflexes were lost in the lower extremities. Died May 9th.

At the autopsy the bullet was found to have taken a direction downward and forward and to have lodged in the body of the second dorsal vertebra, the point directed into the spinal canal, into which it had very nearly penetrated. The cord just opposite the ball was very much attenuated and quite soft for about two inches.

In the first three cases I have reported the patients fell upon their backs, and in these the spine received the concussion at the same time along its whole length; in the fourth case the head struck first, and the shock was delivered to one end of the spine and transmitted along its length. In these cases we may, without much difficulty, conceive of the vibrations of the bones of the spine being transmitted to the cord in spite of the delicate protective mechanism that guards it from jar, and so to have mechanically disarranged its particles as to destroy its functions. But in Case V. the impinging ball struck the side of one vertebra in an oblique direction nearly approaching the horizontal. In this case I confess I find it difficult to conceive of vibrations transmitted along the vertebræ and intervertebral disks of sufficient force to jostle the cord into a reflex impotence.

In the endeavor to explain the loss of the reflexes here, it is useless to invoke anæmia or hyperæmia of the cord, since these alterations of the circulation, if they existed, were but the symptoms of the nervous lesion the nature of which we seek. A temporary inhibition of the reflexes appears to me inconsistent with the length of time it persisted in some of the cases.

It is, of course, mere speculation, but I cannot but think of the nutritive dependence of one portion of the nervous system upon another, and imagine an alteration (inhibition?) of molecular nutrition dependent on the local lesion of the cord at the point the bullet struck.

DISCUSSION.

DR. EDES: I have observed and reported (*Boston Med. and Surg. Journal*, September 21, 1882) several cases in which, a number of weeks after injury to the back, there were exaggerated knee-jerk and ankle-clonus. These symptoms, attributable to a degeneration of the postero-lateral columns, are interesting as showing that there are cases of "concussion of the spine" which are more than hysterical.

It seems to me not improbable that Dr. Miles's cases might, if they had lived longer, have developed an increase of tendon reflex.

In the case of a man who had fallen on his back across the wheel of a wagon, some months previous to his entering the hospital, the tendon reflexes of the upper extremities were much increased, and the limbs soon became extremely contractured. The condition of the lower limbs, owing to œdema and sloughing, could be less carefully examined. It was supposed that there had been a fracture of the spine, but the autopsy showed that there had not. The anterior columns (Türck) at the postero-lateral border were both degenerated. The parts above the medulla oblongata were, unfortunately, not preserved for microscopical examination; but the inspection of the fresh specimens showed no focal lesion visible to the naked eye.

DR. WM. OSLER: I think that as a result of concussion there may be an abolition of the reflexes which may persist for some length of time. Then, in addition to that, there may be a complete abolition of the reflexes from a central myelitis and a destruction of the integrity of the gray matter. I had a case of that kind recently in Philadelphia. The case to which Dr. Edes referred we have all seen.

DR. H. C. WOOD: I have seen a large number of these cases, and have noticed that spinal traumatism, in different cases, has very different results. In some instances, when the shock has been severe, minute hemorrhages occur in the cord, and acute myelitis results. In other instances a subacute myelitis involving the whole cord is produced. Again, I have seen cases in which the symptoms very closely resembled those of spasmodic tabes, and indicated that the degeneration was chiefly confined to the lateral columns. Finally, there are cases in which, although the symptoms may at first be severe, gradual recovery takes place.

Precisely as a blow upon the head may be followed by various cerebral secondary results, so may the secondary results produced by the concussion of the spinal cord vary. In some instances, the symptoms supposed to be the result of disease of the cord are really due to hysteria, and I recall a case in which, after one of the leading railroad companies had paid ten thousand dollars, in accordance with the decision of nine leading physicians and surgeons, subsequently I myself made the diagnosis of hysteria from fright pro-

duced by the accident and the assertions of the doctors, and the woman was restored to health in a few months.

DR. M. ALLEN STARR: It seems to me that in those cases of concussion in which the reflexes are arrested and in which there is no injury post-mortem to the part of the cord that governs the reflexes, there is the explanation on the theory of inhibitory impulses coming from a higher region and transmitted downward which arrest the reflexes. Such a condition exists in hemiplegia. No doubt in many of the cases of Dr. Miles the condition was one of irritation at a higher level, and so inhibition of the reflex. I agree with Dr. Edes that if the patients lived long enough the irritation would have ceased and they would have recovered with exaggerated reflexes.

DR. MILES: I think that the time that elapsed from the injury to the death of the patient precludes the supposition that the reflexes would have returned. The same may be said with regard to inhibitory impulses coming from above. If the reflexes had been lost because of a destruction of the gray matter of the cord by the injury, or by a central myelitis, we should have had at some time degenerative reaction. There was loss of *all* electrical reaction, but there was no degenerative reaction.

CONTRIBUTIONS TO THE ANATOMY AND PATHOLOGY OF THE THYMUS GLAND.

By A. JACOBI, M.D.,
CLINICAL PROFESSOR OF THE DISEASES OF CHILDREN IN THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK; PRESIDENT OF THE NEW YORK ACADEMY OF MEDICINE.

THE pathology of the thymus gland and its literature have been extensively discussed of late by Giuseppe Somma ("Sulla tracheostenosi per ipertrofia congenita del timo," in *Arch. di patol. inf.*, 1884, fasc. 4), and A. Sanné (Art. "Thymus," in *Dict. Encyclop. des sciences méd.*, 1887). Otherwise the literature of the thymus, for the last thirty years, since Alexander Friedleben published his exhaustive and brilliant monograph on "Die Physiologie der Thymusdrüse in Gesundheit und Krankheit vom Standpunkte experimenteller Forschung und Klinischer Erfahrung" (Frankfurt A. M., 1858), has been mostly confined to embryology and histology.

In the following pages, therefore, I venture to offer, with a few remarks and cases, the results of some researches into the anatomical relations, and some pathological conditions of the thymus. The specimens have been prepared and the microscopical examinations made by my young friend, Dr. Henry Koplik, mostly in the laboratory of the College of Physicians and Surgeons of New York. The anatomical material required came from several institutions of the city. It is mainly to the kindness of Dr. W. P. Northrup, of the Foundling Asylum, that I am indebted for most of the available cases, with the reports of both their autopsies and histories.

Of the whole number of cases, thirty-two were carefully examined. Amongst them there were four of tuberculosis, one of which was excluded because both the history of the case and the necropsy were incomplete. There were five cases of syphilis, one of which was thrown out because its history was defective. Three were fully examined. Of diphtheria there were many specimens; nine of them

were also investigated with great care. Besides, there was a specimen of persistent thymus taken from an adult.

Plates I.–VII. were taken from the frozen sections of a newly born normal baby. They were made for the purpose of establishing the exact location of the thymus gland. All the sections were made one centimetre apart, with the exception of Plate II., which is but half a centimetre in thickness. The first and sixth sections exhibit the upper aspect. We look down upon it. In Plates II., III., IV., and V. we look up toward them. The plates are taken from the full sizes, the specimens having shrunken since, say, to five-sixths or seven-eighths of the original size.

Diseases of the Thymus.

Friedleben's conclusions derived from the closest possible study of the whole literature, and many researches of his own, lead to the results that the diseases of the thymus are quite rare, and that the conditions of hyperæmia and apoplexy, besides those depending on violence experienced during parturition, and tuberculous degenerations, have indeed been met with, but very much more rarely than some authors had supposed them to occur. The large majority of observations alleged to be those of diseases of the thymus, were, according to him, no diseases of that or any other organ. He could not ascertain any positive symptoms during life belonging exclusively to such presumed diseases. There was but a single *abscess* of the thymus at this time, and that also exhibited no recognizable symptoms.

Hypertrophy.

The existence of a hypertrophy of the thymus gland has often been both asserted and denied. It is a fact that its weight may vary between a few and nearly five hundred grains, without giving rise to any symptoms whatsoever. According to Friedleben, the thymus, both normal and abnormally large, cannot impede respiration or circulation, or irritate the respiratory nerves, or disturb the cerebral circulation or the innervation of the muscles controlling the glottis, or be subject to a periodic turgescence by impeded circulation. Before him, as early as 1847, Herard reported, in his inaugural thesis, "Du spasme de la glotte," his examinations of the thymus of sixty infants,

particularly with regard to its weight. He found the weight very variable, and that it depended on the constitution of the infant, and the degree of its emaciation, but also came to the conclusion that the bulk would vary considerably both in a healthy condition and in what was called thymic asthma.

West admits the existence of thymic asthma, and reports a single case without attributing to it the same symptoms found in the common forms of spasm of the glottis.

Rilliet and Barthez look upon spasm of the glottis as a convulsive, not a compressive affection. Sanné also makes a difference between the convulsive and the compressive form, and expresses the opinion that when the degenerated thymus or lymphatic glands are sufficiently large to compress the trachea and the adjoining nerves, particularly the inferior laryngeal, the consequences differ from the symptoms of glottic spasm.

Now, the distance, in the skeleton of an infant of eight months, between the manubrium sterni and the vertebral column amounts to two centimetres. Thus it is clear that a thymus gland of the size reported above, particularly in cases of temporary congestion and swelling, is sufficient to fill the whole space, compress the neighboring organs, and result in death. Thus it is quite possible to explain an occasional case of sudden death, and the so-called thymic asthma by an hypertrophy of the thymus gland, though the large majority cannot be referred to such a cause. It is well known that the large majority of cases of laryngismus stridulus or spasm of the glottis have to be explained by changes in the nerve centre. It ought to be taken as an established fact, that most cases are due to meningeal and encephalic hyperæmia and effusion resulting from the changes produced by general rhachitis.

Until about twenty years ago, the several forms of rhachitis were not frequent amongst us. Particularly rhachitical softening of the cranial bones ("craniotabes"), and its cerebral results, were quite rare. Laryngismus stridulus was quite uncommon. It is not difficult to find the reasons for this peculiar fact. At that time the number of very poor people, with all the diseases of abject poverty, was quite small amongst us. Since that time the indiscriminate immigration of hundreds of thousands every year, of the lowest classes of all the nations of Europe, has greatly changed the average social and hygienic

condition, and rhachitis has become quite frequent, with all its consequences. Thus it is that thymic asthma, or laryngismus, has become quite frequent amongst us, though certainly the sizes of the thymus glands have not increased.

Still, sudden deaths, resembling those published by Grawitz, have been observed in a very few instances. One of his cases was that of a child of eight months, who was found dead in bed, after having been in perfect health. At the post-mortem examination absolutely nothing was found to explain death, except a thymus of unusual size, which was flattened, and covered the larger part of the pericardium, and extended upward to an unusual degree in the direction of the thyroid gland.

The second was a babe of six months, in perfect health, which, while being carried on the arm of the father, was taken with an attack of dyspnœa, became cyanotic, and died in a few minutes. At the post-mortem it was found that there was a large amount of subcutaneous fatty tissue, and symptoms of rhachitis about the chest. The thorax was broad, and the abdomen somewhat inflated. The diaphragm reached upward to the fourth rib on both sides. The thymus was very large, its two lobes covering the larger part of the pericardium, and two processes reached upward to the thyroid gland. Longitudinally it measured seven and a half centimetres; over the pericardium it was more than six centimetres wide; its thickness was one and a half centimetres, with the exception of the region of the manubrium sterni, where the dorso-ventral diameter amounted to one and four-fifths centimetres. The tissue of the thymus was of a grayish-pink color, quite firm, and contained a great many punctated hemorrhages. There was a large amount of blood in the heart and the two venæ cavæ. The epiglottis was compressed from the two sides. The spleen was large, and the mesenteric glands were larger than normal.

Goodhart (*Brit. Med. Journ.*, 1879) reports a case of enlargement of the thymus in an infant of eight months, in which suffocatory and epileptiform attacks proved fatal.

Clar (*Jahrb. f. Kinderh.*, 1858) relates eight observations of asthmatic attacks in children, explained by hypertrophied thymus glands. Two were girls of sixteen days and two months in whom death resulted from a sudden swelling of the organ. Two died of pulmonary catarrh which was increased by the hypertrophied gland. In a boy of one

year and nine months, who suffered from croup, the introduction of the tracheotomy tube proved impossible from the same cause.

Amongst a great many cases of laryngismus stridulus, I have met with but a dozen sudden deaths. In but one of those which I could examine after death, I found the cerebral and meningeal changes of rhachitis insufficient to explain the fatal termination. In that one case the thymus of the baby (boy of six months) had a weight of four hundred and ten grains, and I felt justified in attributing the sudden death of the patient to the size of the thymus gland.

Hemorrhages.

Hemorrhages into the tissue of the thymus, and into its capsule, mostly at its inner surface, are by no means rare. Scores of them, of the size of a pin's head and less, may be found in babies dying a few days after birth from, or with, disorders of the scarcely established circulation. Atelectasis, pneumonia, congenital heart diseases, will result in so much venous congestion as to burst numerous small bloodvessels in many organs, principally, but not essentially, of the thoracic cavity.

Péan (*Bull. Soc. Anat.*, 1857, vol. 32, p. 375) describes the case of a thymus very much enlarged, which had the appearance of spleen tissue and presented a number of hemorrhages, varying in size from that of a pin's head to that of a grain of oats. The specimen was taken from an infant who died with hemorrhagic purpura at the age of eleven months.

Cysts.

Bednar met with cysts containing a clear yellowish serum. Some had the size of beans, some were quite large and replaced the lobes of the organ. Were they the results of softened gummata? or not, rather, the results of hæmatomata changed in the manner in which they will be transformed, in the cranial cavity, into pachymeningeal cysts?

Inflammation.

Inflammations of the thymus have been described, but it has been found impossible to distinguish them from their complications. Thus

E. Lancereaux (*Traité d'Anatomie Pathol.*, ii. p. 628) speaks of exudative, suppurative, hypertrophic, tuberculous, and syphilitic inflammations. Two cases of inflammation of the capsules of the thymus have come under my observation, but neither of them was primary. One of them appeared to be secondary to a general mediastinitis and pericarditis. The capsule was very hyperæmic, thickened, and fibrinous deposits were found on it.

The second case was one in which there was a considerable amount of fibrinous pleuritis on both sides. The capsule of the thymus was thickened, covered with thick layers of fibrin, was easily peeled off the organ; the latter was hyperæmic, and contained a few punctated hemorrhages. In this case there were in the neighborhood a considerable number of enlarged mediastinal glands.

It is the opinion also of Sanné, that inflammations of the organ and its capsule are mostly not of a primary character, but the results of difficulties experienced during parturition, and generally attended with hyperæmia, œdema, and hemorrhages in other places. Similar cases have been mentioned by Veron, Billard, and Weber.

Wittich (*Virchow's Arch.*, viii. p. 477, 1855) reports the case of a young man of eighteen, who complained a long time of a retro-sternal pain and intense dyspnœa, particularly when in the recumbent position. After having suffered several months, he was admitted to the hospital, where he died, in an attack of suffocation, with hydrothorax and ascites. There was found at the autopsy bilateral pleurisy with bloody and serous effusion; no tuberculosis; plenty of adhesions; in the left upper lobe, emphysema, in the left lower lobes, atelectasis; the external layer of the pericardium considerably thickened and discolored by pigment. The pericardium at its base was seven inches wide, its height from four to five inches. There was a large tumor which contained some normal tissue of the thymus gland, some cavities filled with pus and surrounded by a hyperæmic zone, other cavities filled with serum, pigmented granulations, and some fat. It could not be separated from the pericardium, the large bloodvessels, and the trachea. Even in this case it is very difficult to say whether the tumor was primary or not, for it is possible that it originated in the pericardium, the external layer of which was affected, or that it was the result of progressive mediastinitis.

Malignant Tumors.

The case of primary *medullary sarcoma* reported by Astley Cooper in a girl of nineteen years is considered as extremely doubtful by Friedleben. Birch-Hirschfeld, however, expresses the opinion that many of the tumors of the anterior mediastinum originate in the thymus.

Steudener (*Virch. Arch.*, vol. 59, p. 463, 1874) reports the case of a child of one year, who died of pneumonia of the right lung. There had been no symptoms previously, but there was found a *hemorrhagic sarcoma* consisting of round cells which originated in the thymus, perforated its capsule in different places, and adhered to the large bloodvessels.

Soderbaum and Hedenius published, in 1878, the case of a man of twenty-two who had been suffering for some time, without a diagnosis of his condition having been made. He had a mild attack of pleurisy and cough, both of which disappeared, but a certain degree of dyspnœa remained, increased gradually, particularly in certain positions. After a while there was œdema of the upper part of the body. There were no attacks of suffocation, but dyspnœa increased, and he died nineteen months after admission. There was a tumor of the thymus quite circumscribed, surrounded by signs of mild vascular irritation, compressing to a certain extent the neighboring bloodvessels and bronchi. It consisted of two parts, the anterior portion containing a large amount of fat and lymph elements, epithelial cells, and the characteristic concentric bodies of the thymus gland. The posterior part contained connective tissue cells in various degrees of development.

Vogel observed two boys of from five to six years of age with a *carcinoma* which, to judge from the position of the tumors, originated probably in the thymus gland.

T. Grützner published in 1869 (*Berlin Inaug. Diss.*) the case of a boy of eight years, whose family history was good, who died with a large tumor, diagnosticated post-mortem as a lymphosarcoma. Immense masses of cells were accumulated in the reticulated connective tissue. Its origin was assumed to be the thymus gland, for the main bulk was uniform in both its texture and outlines, and did not make

the impression of a conglomerate of enlarged and degenerated lymph bodies.

One of two cases described by Rosenberg (*Inaug. Dissert.*, Gottingen, 1884) was that of a boy of five years, who died with a lymphadenoma filling the whole mediastinum. It was closely adherent to the sternum, extended into the right pleural cavity, and was strongly adherent to the lower anterior part of the right upper lobe. It was assumed to be thymic, because no trace of the thymus could be found. Besides, it was homogeneous in its structure and not nodulated like a conglomerate of lymphomata, and was located in front of the pericardium, behind the sternum.

S. Bollag reports (*Inaug. Diss.*, Brugg, 1887) a lymphosarcoma of the thymus, observed in a boy of fourteen years who died in the University hospital of Zurich in 1883. It was located in the mediastinum, originated in the thymus, and resulted in secondary sarcomatous degeneration of the lymph bodies and the parietal pericardium. Moreover, there were hemorrhagic pericarditis, compression of the trachea and of the vena cava superior. There were, in consequence, extensive œdema, bilateral hydrothorax, compression of the lower parts of the lungs, protrusion of the sternum, and dislocation of the liver.

Jules Simon has collected, in his inaugural thesis, a few cases of enlarged thymus implicating *leucocythæmia*. One such case has been detailed by Augustin Fabre ("Fragments de clinique méd.," Paris, 1881). The leucocythæmia was both splenic and lymphatic. There were pleurisy of the left side, œdema of the left upper extremity, considerable dilatation of the veins of the thoracic wall and of the left shoulder. The only symptom perceptible during life was extensive dulness over the manubrium sterni. The latter was the only symptom distinguishable in the single case of the kind that has been observed by me. A child of two years had dyspnœa, dilated veins, œdema of the face and neck, and a high degree of anæmia. The tumor was taken for a mediastinal growth. The diagnosis of leucocythæmia was made, fortunately, before death, when the spleen was found greatly enlarged. The thymus was changed into a lymphosarcoma, and some of the mediastinal glands were altered in the same manner.

Persistent Thymus.

Persistent thymus has been described occasionally, thus, for instance, by Alexander Bruce ("Specimens of thymus remaining to a later period of life than normal," in *Trans. Pathol. Soc.*, 1867, vol. xviii. p. 263). Before him, Haugsted observed a persistent thymus in a boy and girl of ten years, and a young man of seventeen, and Krause, in two men of twenty-five years, and in a woman of twenty-eight. Though there be a number of other observations of the kind, the examination of a case of persistent thymus which came under our notice will still be found interesting.

Persistent Thymus in the Adult.—The above was removed from the body of a male, aged twenty-six years, the cause of whose death seemed to be unknown. At least, no positive lesion was found post-mortem. Dr. Van Gieson, of the Pathological Laboratory of the College of Physicians and Surgeons, of New York, furnished this specimen and kindly allowed us to make sections and study the same.

When found, it appeared as a small, pinkish tumor in the anterior mediastinum in front of the pericardium. It weighed, when hardened, 11.50 grammes. Macroscopically it appeared, on section, to be composed of yellowish, firm islets of tissue, separated by tissue less firm and more whitish in color.

When double-stained the above firmer areas (lobules) must be recognized as the remains of the thymus parenchyma; they were surrounded by fibrillar connective tissue from which trabeculæ were given off dividing these areas after the manner of acini or follicles. The tissue in these areas was composed of a mass of small and large round cells. In spots there were spindle-shaped cells, the whole having a finely fibrillated basement-substance.

The small round cells were so closely packed as quite to conceal the basement-substance. The whole was much firmer than generally normal to the thymus. In the midst of the acini were met the characteristic concentric bodies found in the normal thymus. Some of these showed calcific changes, others still maintained their characters of a capsule with concentrically arranged layers of epithelioid cells.

The bloodvessels of smaller size in the acini were in places found to have undergone obliterating changes. In some places the coats were

infiltrated with round and spindle-shaped cells. The lumen was filled with connective tissue.

The tissue between the above-described areas was in every way identical with adult fat tissue (see Plate VIII.).

Diagnosis: the connective tissue originally separating the acini of the thymus was replaced by fat tissue. The parenchyma could be distinctly seen to have yielded to the invasion of fat tissue. In places only a small area of round cells (appearing as if they infiltrated the fat tissue) was all that remained of the original acinus of the thymus. The characters of the above persistent areas correspond to the picture given in the normal thymus, allowing for physiological changes.

Tuberculosis of the Thymus Gland.

The literature of tuberculosis of the thymus gland is quite scanty. Indeed, there is hardly any. In the *Études expér. et clin. sur la tuberculose*, published under the direction of Prof. Verneuil (1887 and 1888), it is not even mentioned. The second edition of H. Hérard, V. Cornil, and V. Hanot's *La phthisie pulmonaire* (1888) contains a few remarks on tuberculosis of the thyroid body, but no mention is made of that of the thymus. And A. Predöhl's *History of Tuberculosis* (Hamburg and Leipzig, 1888) refers to no instance of tuberculosis of the thymus besides the case reported by Demme, to which I shall allude below.

Sanné quotes Harder, who describes a blackish and indurated thymus in a boy of fifteen years, who died of consumption; and Budaeus, with the report of a man who died at thirty-two years of age, with an enlarged and "scirrhous" thymus. But most of the old observations suffer from insufficient observation and examination.

It appears that mistakes in regard to the diagnosis of tuberculous changes in the thymus gland are quite possible. The neighboring lymph bodies are certainly much more liable to undergo tuberculosis than the thymus. Probably the case of primary invasion claimed by Bednar (*Die Krankh. d. Neugeb. u. Säugl.*, 1857, p. 94) to have taken place in the thymus, means, indeed, a tuberculous swelling of an adjoining gland. I have met with exactly such cases, in which the thymus was reduced to a very small size by being compressed by a large tuberculous tumor in the neighborhood. In such a case the

atrophy can be so considerable that nothing appears to remain but a thick layer of capsular tissue closely adhering to the tumor. Still, Vogel and Bednar report the observation of large "tubercular" tumors without any tumefaction of the neighboring glands.

There is but a single well-authenticated case of isolated primary tuberculosis of the thymus in modern literature. It is that of R. Demme, published in the *Twenty-second Annual Report of the Children's Hospital in Bern*, 1885. The case is that of a newly born male, the child of non-tuberculous parents. He weighed 2780 grammes at birth, and was nursed at the breast of the mother for three weeks; afterward, on cow's milk. From that time on he became emaciated, suffered from diarrhœa, and showed all the symptoms of general debility; there was absolutely no physical symptom except some dulness on percussion over the manubrium sterni, until he died on the forty-second day. The thymus was found to be quite large, and its tissue dense. In it there were three tubercles of the size of a pea each, and one of the size of a hazelnut. In these tumors some tubercle bacilli were found. In no other organ were there any tubercles or cheesy degenerations.

CASE I. *General tuberculosis; meningitis.*—Joseph B., aged seven and a half months.

January 27, 1888. Returned to asylum from his private domicile. Has been restless for three days; obstinate constipation. Temp. 99½°.

28*th*. Temp. 102°; evening 104°. Resp. irregular. Sordes on lips; fontanelle elevated; abdomen retracted; reflexes good. No convulsions; neck not stiff. Pupils not dilated; respond to light. No cough. Inclined to stupor. Quiet at night. Restless last night and this A. M. Is teething.

29*th*. No constipation. Temp. 100°, pulse 130, resp. Cheyne-Stokes. Eyeballs twitching; eyes half closed. Face flushed. Tâche cerebrale absent.

30*th*. Stupid. Dulness at upper part left side. Pupils slightly contracted; conjunctivæ congested. Marked Cheyne-Stokes resp. Temp. 103°, pulse 196. Eruption over abdomen. No convulsions; neck stiff. P. M. temp. 107½°. Died 10 P. M.

Autopsy. 31*st*, 1 P. M.—Body well nourished; fontanelles elevated.

Brain: Ventricles much distended by serous fluid (℥iij). Convolutions flattened. Fine tubercles everywhere along vessels and over base; also choroid plexus; œdema and infiltration of pia mater at base.

Spinal cord: Tubercles on dura.

Lungs: Adhesions at left apex, and beneath is a fluctuating cavity filled with pus and detritus. Scattered miliary tubercles over surface and on left pleura and left diaphragmatic pleura.

Bronchial glands enlarged; tubercular; some cheesy. Liver not large; miliary tubercles discrete. Spleen not large; miliary tubercles discrete Kidneys, miliary tubercles discrete. Mesenteric glands moderately enlarged; miliary tubercles. Heart, normal. Heart, ʒj; liver ʒvijss; spleen, ʒvss.

The portion of the thymus which was hardened in alcohol and was taken from the anterior aspect of the organ, contained an area of grayish-white color more compact than the remainder of the thymus tissue. This area (measuring five mm. by two mm.) occupied that portion of the thymus anteriorly just beneath the capsule, but in the tissue substance proper of the thymus.

Sections stained with hematoxylon and eosin and picro-carmine showed that it was wholly made up of a number of miliary tubercles (a recent eruption) taking up the whole space allotted to an acinus of the thymus. Each tubercle granulum consisted of a giant cell, around which were arranged spheroidal and polygonal cells. There was no area of cheesy degeneration seen. The spaces between these formations were filled with small round cells.

The walls of the smaller and medium-sized arteries, both in the above area and in the neighboring acini of the thymus, were the seat of an endarteritis of a tubercular character.

The adventitia and media were infiltrated with round cells; the lumina of the smaller vessels were obliterated. In some cases the lumen was replaced by a mass of small round cells. The septa of the thymus in different parts of the organ were infiltrated with tubercle tissue, and the vessels of small calibre were also the seat of tubercular changes.

Tubercle bacilli were found in the giant cells of the miliary tubercles, in the centre and periphery of the tubercle granula, in the spaces between the cells of the tubercle. They were present also in the walls of the bloodvessels which were the seat of endarteritis. They were found in the lumina of some of these vessels. They existed in the connective-tissue septa separating the acini of the thymus, which septa may be the seat of an infiltration of tubercle tissue. In this thymus tubercle bacilli were found in those encapsulated nests of epithelioid cells which exist in the thymus and are characteristic of its structure. Here the tubercle bacilli were seen in the connective-tissue capsule and between the cells in the interior of these bodies. This was the case in the acini, the seat of eruption of miliary tubercle, and also in places quite distant from the same. Tubercle bacilli were found between the lymphoid cells (in lymph spaces) of the thymus, quite distant from the miliary tubercle in acini apparently not yet the seat of infiltration with tubercle tissue.

CASE II. *General tuberculosis.*—Jennie A., aged eight months and nineteen days. Entered asylum June 10, 1887, three weeks old. Condition poor. Had snuffles. Wet-nursed up to January 24, 1888, when she was returned from boarding; then bottle-fed. Condition: Emaciated; enlarged cervical glands; did not improve. Died February 8, 1888, 10 A.M.

Autopsy. Feb. 8, 2.30 P.M.—Body poorly nourished. Right lung: tubercles; also over right pleura, along posterior margin. Bronchial glands lying against right lung are tuberculous. Left lung and pleura normal. Mesen-

teric glands enlarged; one tuberculous. Agminated glands of intestine enlarged. Liver and spleen tuberculous. Kidneys normal. Body, 8½ pounds; liver, ℥vij ℥ivss; spleen, ℥ivss; heart, ℥vijss; thymus, ℥j grs. xviij.

The thymus was hardened in Müller's fluid and alcohol. On cutting the thymus two nodules of a cheesy character, on close examination, were found in the lateral portion of the organ and within its structure. One of these nodules was quite large, the size of a bean, the other much smaller. Under the microscope, the above nodules were found to have a large, cheesy centre, and a periphery composed of a number of tubercle granula, which contained giant cells, around which were found spheroidal or polygonal cells, both small and large; between these granula were small and large polygonal cells (tubercle tissue). The nodules were limited to a sort of connective-tissue capsule which belonged doubtless to the limiting connective-tissue septa of the acini of the thymus.

In some of the tubercle granula above mentioned there were cheesy areas (coagulation-necrosis). The septa limiting the acini in various parts of the thymus were infiltrated with tubercle tissue. The bloodvessels of smaller size in these situations were the seat of an endarteritis of a tubercular nature. The adventitia and media of some of the medium-sized vessels were in a state of hyaline change, and infiltrated with small round or spheroidal cells. In some places the smaller arteries were obliterated, the lumen having been replaced by spheroidal or epithelioid cells.

Sections of the thymus were placed in alcohol, and stained by the Koch-Ehrlich method, both double and single stain. Tubercle bacilli appeared in the areas of coagulation-necrosis, in the tubercle granula, both at their centres and peripheries. The walls of the bloodvessels were the seats of tubercular endarteritis. Tubercle bacilli were present in the tubercle tissue infiltrating the septa of the thymus in various parts of its extent. There were tubercle bacilli in places within the acini apparently free from the presence of any distinct tubercle formation. The bacilli were situated in the spaces between the lymphoid cells of the thymus.

CASE III. *General tuberculosis; convulsions.*—Marion W., aged four months. January 17th, put out to wet-nurse; nurse's health failed; then bottle-fed after the middle of February. April 17th: Slight cough: constipated; head retracted; slight convulsive twitchings. 19th: Quieter; green undigested passages. Returned April 25th in convulsion; collapse followed. Died April 26th, 1 A.M.

Autopsy. 2 P.M.—Body well nourished. Brain normal. Double bronchopneumonia, chiefly on right side; some tubercles; bronchial and tracheal glands enlarged, cheesy. Tubercles in *pleura, liver, and spleen.* Kidneys, markings slightly indistinct. Mesenteric glands enlarged. Intestines pale, empty (from oil and injections). Peyer's patches enlarged. Body, 12½ pounds; heart, ℥j ℥vss; liver, ℥viij ℥ij; spleen, ℥iijss; thymus, ℥iij grs. xj.

The thymus was hardened in Müller's fluid, and portions in alcohol. The organ showed on section the presence of a number of small grayish-white bodies occupying the anterior portion of the thymus. The tissue between

these bodies (miliary tubercles) appeared denser and darker in color than normal. On microscopical examination, these bodies were found to be an eruption of miliary tubercles. These were composed entirely of small round or polygonal cells; at the centre of some of these collections or areas there were cheesy changes (coagulation-necrosis). In the periphery of these miliary tubercles the basement-substance showed fully well as a fine reticulated tissue forming spaces in which the round cells were found in other cases empty (per drawing). The tissue between the miliary tubercles was composed of round cells closely packed, in no way differing from the normal lymphoid cells of the thymus. They were more numerous, however. The smaller bloodvessels in but very few instances could be found the seat of endarteritis.

A careful examination found tubercle bacilli (as in drawing) in *moderate* numbers in the cheesy centres, and in the periphery of the miliary tubercles, and also present in the tissue between the miliary tubercles.

METHODS OF INVESTIGATION.—The hardening fluids used were Müller's fluid and alcohol, simple alcohol, corrosive sublimate, chromic acid (one-half per cent.). Those portions of the thymus which were hardened in corrosive sublimate at a temperature of about 48° C. (118° F.) for nearly fifteen minutes and then placed in alcohol, first dilute and then strong, became quite brittle, and though they cut fairly well, they stained very badly with hæmatoxylin and eosin, or picro-carmine. The most satisfactory methods were alcohol or Müller's fluid. Paraffine and celloidin were used as embedding material, the latter in most cases. For tubercle bacilli, the methods recommended by Koch in his laboratory, and the modifications of the same by Ehrlich were made use of—methyl-violet, fuchsin, and Bismarck-brown being used either singly or conjointly; the sections being decolorized always in nitric acid, as recommended by Koch.

In simple staining, the fluids used were hæmatoxylin (Delafield) with eosin, carmine, picro-carmine, safranin, methyl-violet, fuchsin.

Specimens were always studied in both glycerine and balsam.

CONCLUSIONS.—In the cases of tuberculosis of the thymus here presented, tubercle tissue appeared in the following forms:

1. As miliary tubercles, composed entirely of small round or polygonal cells with a reticulated basement-substance in the recent state.

2. In the later stages, these miliary tubercles or granula may, at their centres, undergo cheesy metamorphosis (coagulation-necrosis).

3. Miliary granula are also found which show, in their centres, the presence of giant cells.

4. Large cheesy areas, in the periphery of which we find still miliary tubercles or granula composed of giant cells, around which are arranged spheroidal or polygonal cells in a fine reticulated basement-substance.

5. In all cases the arteries of the adjacent areas of the thymus tissue were the seat of a typical endarteritis (in some cases obliterating) of a tubercular character.

6. A very careful and painstaking examination of the above forms of tuberculosis of the thymus, showed that the bacillus tuberculosis was present in all cases. In most of the above forms we had also the presence of the bacillus in the walls of the arteries and arterioles undergoing tubercular changes, and also in the lumen of the vessels with more or less obliterating changes.

7. In the thymus, tuberculosis may appear simply as so-called tubercle tissue, an infiltration of the tissue of the gland or organ with spheroidal or polygonal cells held together by a delicate basement-substance; this tissue has no characteristic arrangement; the arteries in such areas may be the seat of obliterating processes, and in all the cases examined by us there were present the characteristic bacillus tuberculosis.

Diphtheria of the Thymus.

The thymus glands from nine cases of diphtheria were examined. They were cases of diphtheria of the fauces, or larynx; and in all of them the diphtheria was extensive, and in some was accompanied by broncho-pneumonia. Autopsy, as can be seen in the histories, showed enlarged bronchial and cervical lymphatic glands. The methods of study in all of these cases was the same, on the whole, as that pursued in the cases of tuberculosis. In only two cases were any marked changes found. In the remaining cases the changes from the normal were not evident on microscopical examination. In the thymus from which Plate XIII. was made, the appearances sketched were scattered in small areas throughout the thymus. These areas were less consistent, and when hardened it was more difficult to obtain sections of them than of the rest of the organ.

Though the thymus was well hardened, the portions in which the picture (Plate XIII.) was duplicated seemed to be undergoing a

beginning disintegration. The other case in which similar appearances were found was a severe fatal diphtheria with sepsis at the umbilicus.

The appearances are as follows:

In the medullary portion, or sometimes the zone of the cortical portion of an acinus bordering on the medullary portion, there were found irregularly shaped areas, of a structure distinctly different from that found normally, and staining differently with hæmatoxylin or fuchsin from the surrounding tissue. With hæmatoxylin and eosin they stained of a dirty pink hue.

These areas were distinctly bounded by a narrow zone of small round cells, not differing in any way from the lymphoid cells commonly found in the normal thymus. The areas themselves consisted of irregular, large, coarsely granular cells, resembling large epithelial cells; between these were small round cells, granular detritus, and free large, irregularly shaped nuclei.

In these areas bloodvessels were distinct. Some cells contained more than one nucleus, others showed the nuclei irregularly shaped or constricted. Some of the cells contained large vacuoles. Making due allowance for the effects of hardening fluids, it was difficult not to surmise that some of these queer-shaped nuclei were the effect of a beginning process of division (karyokinesis). The bloodvessels showed no changes.

These areas were very plentiful in this thymus. The thymus, on the whole, reminded one very much of the pictures given by Oertel of changes in the lymphatic glands in cases of diphtheria (necrobiosis), though not so advanced as in the cases examined and pictured by him.

One of the cases examined was that of a newly born infant. I give a few particulars because of the comparative infrequency of diphtheria of the newly born.

CASE IV. *Diphtheria; sepsis.*—Hilda Myers, born February 28, 1888. Mother showed signs of septic infection March 3d, followed by pelvic abscess.

March 5. Child. Umbilicus, redness size of a silver dollar. Infiltration extending nearly to ensiform appendix and pubes. Slightly bloody mucous discharge from the nose. Pulse 90.

6*th*. A.M. Infiltration decreasing, softening of the right side nearly to umbilicus. Throat red, no exudation. Pulse 100. P.M. White patches on the tonsils and pharynx. Pulse 90.

7th. Exudation in the throat increased. Pulse, A. M., 100; P. M., 96. Respiration, A. M., 40; P. M., 40.

8th. Does not swell. Cyanosis marked at times. Respiration irregular. Infiltration extended over abdomen. Pulse, A. M., 100; P. M., 112. Respiration, A. M., 40; P. M., 40.

9th. Died 6.30 A. M.

In this case the temperature ranged from 99½° March 5th, to 104° March 6th, it then dropped to 99¾° March 7th A. M. It rose again to 102¾° March 8th A. M., then dropped suddenly to 97°, the temperature at time of death.

CASE V. *Diphtheria with nephritis and convulsions.*—Frederick S., aged three years, twenty-two days. Returned from boarding April 16th. Soon appeared ill; some amygdalitis.

April 22. Hoarse, slight cough.

26th. Tonsils very large. Membrane over tonsils and pharynx. Swelling of the cervical glands.

28th. Has steadily grown worse in the throat and general condition. 4 A. M. convulsions. 4.30 A. M., died.

Autopsy. 28th, 1.30 P. M.—Body well nourished. Pericardial fluid red, but no blood corpuscles to be found.

Tonsils the seat of diphtheria; sloughing, membrane in pharynx, œsophagus and larynx and trachea. Catarrhal bronchitis; a good deal of pus. No pneumonia.

Liver fatty.

Bronchial glands enlarged.

Kidneys, markings indistinct, cortex swollen, pale. Urine (P. M.), granular and hyaline casts.

Body, 25 pounds; heart, ℥ij ʒiiiss; liver, ℔j ℥viij; spleen, ℥ij ʒviiss; thymus, ʒiij gr. iij.

SYPHILIS.

Parrot, who had an extensive experience in syphilis, and for that reason and because he was rather given to overestimating the ravages of that disease than otherwise (according to him, rhachitis is a disorder derived from hereditary syphilis), cannot be expected to have overlooked it where present, denies ever having met with syphilis in the thymus. The reddish or yellowish nodules he often found in it, consisted of connective tissue undergoing fatty degeneration in the normal process of retrogressive metamorphosis. L. Fürth, however, found seven cases of thymus, which he claimed as syphilitic, in two hundred autopsies made on infants who died with congenital syphilis. In such cases the occurrence of tangible changes, such as gummata, is not required. The characteristic changes of the bloodvessels, which

may terminate in vascular obstruction, or in hemorrhages, are sufficient to establish the diagnosis.

There are two cases of hemorrhage of the thymus gland in the *Archiv für Kinderheilkunde*, vol. 4, p. 21, 1883. In one case the mother was known to be syphilitic. The child developed normally for forty-five days, with the exception of a cephalhæmatoma and a smooth nævus on the abdomen. On the forty-fifth day it was suddenly taken ill. There was no albuminuria, but the epithelial "pearls" on the hard palate were found to be stained with blood. Thus the infant died. The left lobe of the thymus was enlarged. Near its hilus was a triangular area of whitish tissue, adjacent to which the tissue was tinged with blood and of a grayish-red color. On its edges the tissue was very smooth and purple. There was blood in the pleural cavities, and recent coagulations in the temporal and occipital fossæ of the cranium.

In the second case the mother was unknown; the baby developed normally until the tenth day. At that early age there were some symptoms of rhachitis. Besides, there were irregular, contracted cicatrices on the skin. The left lobe of the thymus was quite large. In it there was a hemorrhage of the size of a walnut. In the surrounding cellular tissue and on the hard palate there were punctated hemorrhages. There was blood in the stomach and on the surface of the kidneys.

Similar cases are reported by F. Weber in the thymus of the fœtus and the newly born, and there were found general hyperæmia, hemorrhages of the size of a pin and upward; besides, there were hemorrhages in other places, isolated, or a great many in the same locality.

Abscesses of the thymus depending on primary inflammation appear to be very rare. Normally, there are sometimes found accumulations of a milky fluid which is, or may have been, taken for pus. In the large majority of cases in which abscess has been reported, there is also the history of a constitutional disorder. Paul Dubois published a report of four syphilitic fœtuses with disseminated deposits of pus in the thymus gland. Haugsted has the report of two young men, syphilitic and tuberculous, in whose thymuses, which were persistent, pus is said to have been found. Mewis (*Zeitsch. f. Geburtsh. u. Gyn.*, 1879, iv. p. 57), has the following cases: one was that of a fœtus stillborn in the eighth month of utero-gestation. The mother was

reported to have been in good health, but in the centre of the placenta there were several firm yellowish nodules of the size of a hazelnut or less. In the fœtus there were gummata of the lungs, liver and spleen were enlarged, the pancreas was indurated, the mesenteric glands large, and there was found a syphilitic ostitis. There were also a discoloration and a swelling, sometimes amounting to stenosis, of the intima of the veins and arteries, such as are found in syphilis. The thymus was of its normal size, its tissue denser than normal, containing an abscess. No pustular syphilides.

The second was a male fœtus born dead at the end of the eighth month. The history of the mother pointed to syphilis, and in the fœtus there were syphilitic pemphigus, gummata in the lungs, spleen large, pancreas large and indurated, suprarenal capsules large and hemorrhagic. There was syphilitic ostitis. The thymus was large, contained a large number of small abscesses of the size of a pea, with pus of yellowish-green color and thick consistency. Similar observations had been made by Zeissl in cases complicated with syphilitic pustules.

The following cases of ours were examined:

CASE VI. *Congenital syphilis; broncho-pneumonia.*—Josephine C., aged three months and three weeks. Admitted November 2, 1887, when three weeks old. Was then in poor condition; conjunctivitis (syphilis?). Returned from boarding January 9, 1888. Emaciated; liver enlarged; spleen slightly enlarged; snuffles; enlarged cervical glands. Continued to fail; died February 2, 1888, 10 A.M.

Autopsy. Feb. 2, 1 P.M.—Body poorly nourished; broncho-pneumonia left lower lobe; cervical glands enlarged; liver enlarged; spleen slightly enlarged; bronchial glands enlarged. Body, 6¾ pounds; heart, ʒv; liver, ʒvj ʒij; spleen, ʒvss; thymus, ʒss.

CASE VII. *Congenital syphilis; enteritis.*—Arthur W., admitted January 30th, aged two months. Sore mouth; ulcers about genitals; snuffles. Syphilis? Bottle-fed; failed.

Feb. 13. Stupid, slight cough, neck stiff, enlarged inguinal and cervical glands.

14th. Pulse 160; temp. 102°; diarrhœa; pupils contracted.

15th. Some symptoms of meningitis. Abdomen retracted, flabby, doughy; neck stiff and held back, slight spasmodic action of arms, but respiration regular, 23. Pulse 132; temp. 99½°; passed a little blood with stool yesterday.

16th. Died at 3 A.M.

Autopsy. 2 P.M.—Body very poorly nourished; lungs negative. Bronchial glands not enlarged. Mesenteric glands slightly enlarged, not tubercular.

Inguinal and cervical glands as above. Brain normal. Intestines: contents thin and watery with mucus. Body, 6½ pounds; heart, ʒv; spleen, ʒiijss; liver, ℥iv ʒj; thymus, grs. lxiv.

In this thymus the following growth was found. Macroscopically it appeared as a spheroidal mass one and a half mm. in diameter, grayish-yellow in color. It was limited by a distinct zone, which under the microscope was composed of fibrillar connective tissue in which were spindle-shaped and round cells. The central portion of the mass consisted of a hyaline area occupying from one-third to one-half the diameter of the growth. This hyaline mass stained diffusely with eosin and hæmatoxylin, and contained a few oval nuclei situated at the sides of empty oblong spaces; these spaces, which were non-communicating, contained in places detritus. The nuclei and spaces indicated capillaries which were now almost obliterated by this process of hyaline degeneration. Around this central hyaline mass and occupying the space between it and the connective-tissue limiting zone, was a mass of spheroidal cells among which were very thin-walled vessels. In places where the small round cells were not so compact a transparent reticulum containing spindle-shaped cells could be made out. The above round cells were smaller and stained more deeply with hæmatoxylin than those in the acini of the thymus.

Diagnosis.—In the absence of any positive presence of a similar growth in other parts of the body, the nature of the above can only be surmised. Tuberculosis being excluded, the assumption that the above is a gumma in process of formation appears plausible.

The sections show in places an absence of that distinct marking into follicles seen in the thymus. In other places these markings are distinct, though fading off into the surrounding connective tissue. This changed appearance is caused by the presence of an excessive amount of connective tissue as compared with the parenchyma.

The parenchyma is much reduced by the encroachments of the connective tissue. This connective tissue when stained appears more transparent than the fibrillated connective tissue in other parts of the organs, where the markings of the acini are still distinct. The encroachments of connective tissue are first seen in the cortical part of the follicle. This connective tissue, apparently of recent formation, is composed of a transparent finely fibrillated basement-substance containing spindle-shaped and round cells.

Fat tissue is present to a marked degree in the midst of the parenchyma of the acini.

The bloodvessels show similar changes to those seen in another case.

CASE VIII. *Congenital anasarca; syphilis (?).*—James D., seven months miscarriage; lived fifteen minutes. Mother had severe albuminuria and miscarriage as above, 3 A.M., February 29th; child lived fifteen minutes, and breathed eight or ten times. Placenta pale (œdema?). Abdomen greatly distended; anasarca neonati.

Autopsy. Feb. 29, 2 P.M.—Abdomen contained about eleven ounces ascitic

fluid, clear amber; body œdematous, small hemorrhages everywhere in the skin and in the tissues; blue veins over the abdomen. Development good. Liver large. Right lobe contained small lime concretions found by the microscope to lie in the veins. Kidneys: small lobulated hemorrhages. Spleen large. Lungs: partial aëration. Body, ℔. iv ℥j ℥j; liver, ℥ij ℥vss; heart, ℥iijss; spleen, ℥vss; thymus, grs. xxiv.

In this thymus, as in Case VII., the amount of connective tissue far exceeds the area occupied by the tissue in the follicles or acini. Not only is the connective tissue (fibrillar with spindle-shaped and round cells) marked in the spaces between the acini, but it is seen to encroach markedly on the tissue of the follicle, and in places replace it to a large extent. The blood-vessels of the connective-tissue septa or the acini, present no changes other than seen in Case VII.

The parenchyma of the follicle presents nothing pathological but the presence of connective tissue forming, or in an advanced state, giving the appearance as if the follicles were traversed by bands of a tissue composed of a finely fibrillated basement-substance, containing a large number of spindle-shaped and spheroidal cells.

CASE IX. *Congenital syphilis; opisthotonos.*—Irene C., age six months.

March 6. Always in poor condition, but has been failing since yesterday; constipated when admitted six weeks ago; not constipated now; yesterday, slight cough, rapid pulse. Physical examination: Slight dulness on the left side behind lower lobe; bronchitis on both sides, not marked. Yesterday and to-day slight vomiting, restless, erythema over the chest and face. P.M., temperature 103°. No enlarged glands.

7th. Rested well, nursed well.

9th. Opisthotonos; marked at times. Temperature last night, 104.6°. This A.M., 103.2°. Cracks and fissures about the mouth (eczema?). Very restless; bowels irregular, stools white and constipated; vomiting. Nurses. P.M., marked opisthotonos. 10.15 P.M., opisthotonos extreme and continuous, thighs and legs flexed strongly; no loss of sensation apparent. Neck very stiff.

16th. Enlargement of glands in left submaxillary region. Temperature slightly elevated.

18th. Pale red eruption over face and the whole body, especially the legs, soon fading from body and extremities. Opisthotonos continues in spite of opium.

21st. Opisthotonos has gradually increased, head very much extended, eruption faded somewhat, but returned again, and again faded almost entirely. Glands in the left submaxillary region have increased greatly in size. Temperature remains slightly elevated, no abscess found.

23d. Died 11.30 P.M.

Autopsy. *24th*, 1 P.M.—Body emaciated; no rigor mortis. Lungs: Bronchopneumonia left side, lower lobe. Liver congested. Bronchial glands enlarged, not cheesy; mesenteric glands enlarged; inguinal glands enlarged; cervical

glands enlarged, especially on the left side. Kidneys congested, markings distinct, slightly enlarged, firm.

Urine P.M., no casts. Tabes cranii, beaded ribs, fontanelle very open, internal ear on each side normal. Bone behind the external ear on the left side over a small area (one-quarter inch) bare. No abscess cavity.

Brain normal, spinal cord normal; long bones, articulations congested, but condition not absolutely diagnostic of syphilis, though suspicious. Body, ℔. vj ʒxv; heart, ʒvss; liver, ʒv ʒj; spleen, ʒvj; thymus, grs. xlj.

The amount of connective tissue in this thymus, as compared to that generally seen in the organ normally, is quite marked, and the follicles give the gross appearance under a low power, as if blending or fusing into one another. They have a closely packed, flattened appearance. The acini are traversed by transparent bands of connective tissue; this connective tissue is of a fibrillated character, and contains spindle-shaped and round cells. The basement-substance has a transparent, finely fibrillated appearance. There are areas in which the above connective tissue has almost entirely replaced the parenchyma of the follicle.

The bloodvessels show the changes seen in organs which are the seat of interstitial connective-tissue increase. The walls are all uniformly thickened (see remarks). The adventitia is, in many vessels of small size, surrounded by a zone of connective tissue containing spindle-shaped and round cells, arranged concentrically (this is seen in thymuses taken from subjects dying from other causes than syphilis). The above changes in places are more marked in the cortical zone of an acinus.

CASE X. *Congenital syphilis* [?]; *diarrhœa* —Medora C., aged seven months and five days, returned from wet-nurse February 3, 1888; emaciated; afterward bottle-fed. Cervical and inguinal glands enlarged. Liver and spleen appear normal in size. Continued to fail slowly; diarrhœa occasionally.

April 17. 6 A.M., died.

Autopsy. 17th, 2 P.M.—Body emaciated; lungs normal; spleen normal; liver slightly large, congested; heart normal. Kidneys appeared normal, except that they were much congested. Peyer's patches slightly swollen and congested. Cervical and inguinal glands enlarged; mesenteric glands enlarged; bronchial glands not enlarged. Body, 6¾ pounds; heart, ʒ vss; spleen, ʒiv; liver, ʒv ʒivss; thymus, grs. xxivss.

REMARKS.—The appearances, both as to weight and size of the thymus being so variable at different ages, it was attempted to discover whether the thymuses taken from infants at the same, or about the same ages as those taken from syphilitic subjects presented the same, or nearly the same, appearances under the microscope. In many cases so examined the ages not only equalled those of the syphilitic children, but were some of them either younger or older (one, five and a half years). In no case, whether the thymus was taken from a newborn

infant, or one dying with tuberculosis or diphtheria, could exactly the same appearances be duplicated.

Reference is now more particularly had to the immense quantity of connective tissue in the thymuses taken from syphilitic infants whether the thymus be small or large. In one case in which the history simply suspects syphilis, the changes already described were duplicated, but to a very marked degree. In one of the cases the thymus was taken from a premature child (a miscarriage of seven months). The thymus was very small, and weighed twenty-four grains, but showed the most marked amount of connective tissue, as seen by the description.

The bloodvessels in all the cases examined did not show changes limited to any particular coat of the vessel, but a general thickening of all the coats. In some places, both in the interstitial septa and follicles, the vessel was found simply replaced by a ball of connective tissue. But between these two forms no intermediate forms were seen. That is, no vessel was found in which the intima was the seat of thickening, as seen in the cerebral or other arteries which are the seat of endarteritis obliterans. The changes seen resemble those found in chronic interstitial inflammations of the liver or kidneys.

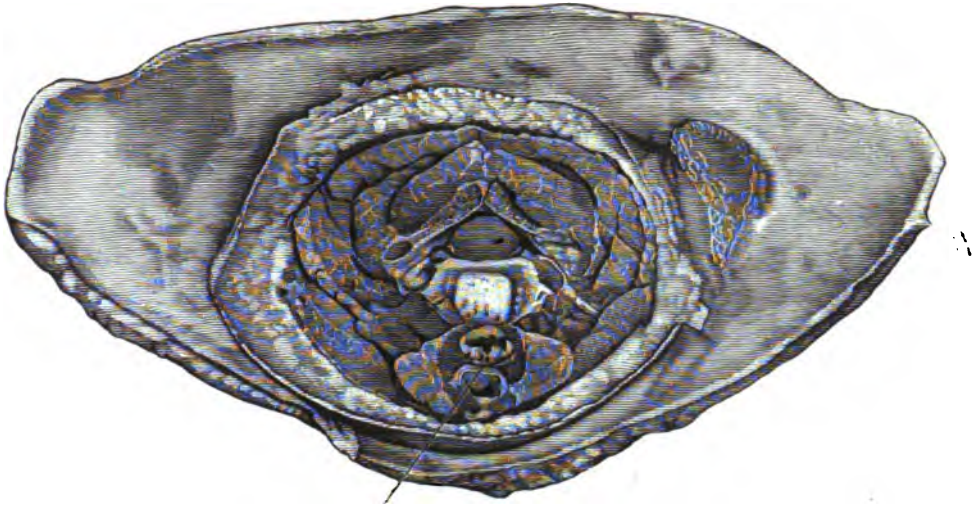
FROZEN SECTIONS OF THE NEWBORN INFANT.

PLATE I.

In this section the cut passes just beneath the cricoid cartilage in front, and behind the *fourth cervical* vertebra is seen.

The plate being photographed from the inferior cut—that is, the section which lay beneath the knife, the body being upright—we must imagine ourselves looking down upon the cut body. If we turn the vertebra toward us, the right of the subject corresponds to the right of the plate. In front is seen the trachea cut just below the cricoid cartilage surrounded by the lobes of the thyroid gland on each side. The thyroid stretches here in front and behind, encroaching also on the œsophagus, which is directly behind the trachea. The œsophagus juts directly on the vertebra, which is seen to be cut at the intervertebral cartilage, exposing its body. Outside of all these parts is seen the skin, which just covers the shoulders of the infant. The relation of the larger vessels is best seen in the actual specimen.

PLATE I.



Thyroid.
Trachea.
Œsophagus.

PLATE II.

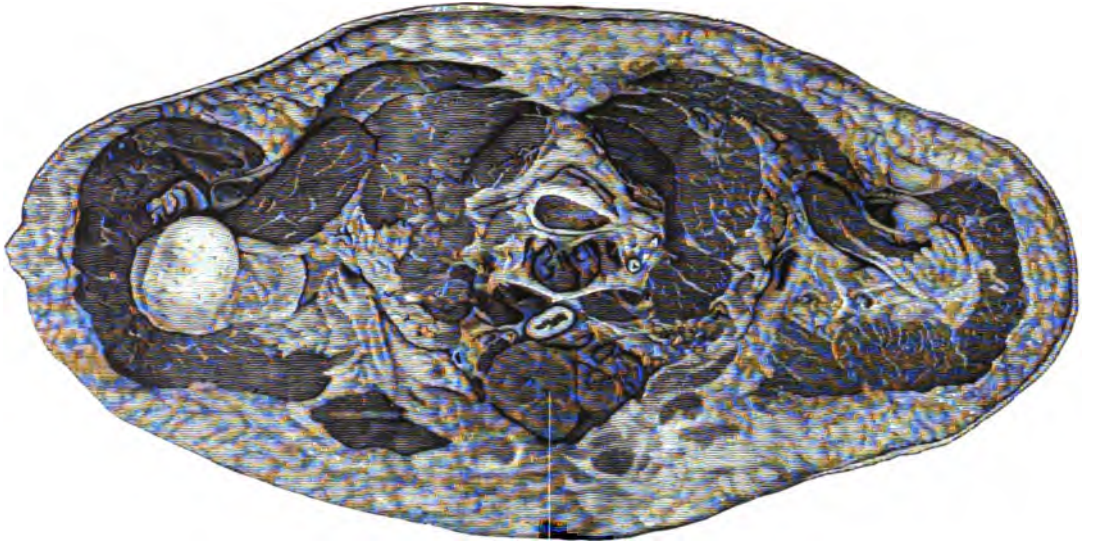
Imagining the body upright, this section lays *above* the knife. It was, therefore, facing downward; the parts being turned upward and photographed, we must consider ourselves looking at the section from below upward. Holding the section in the same position as in Plate I., the right of the subject is to our left, and the left of the plate.

The tops of the heads of the humeri are seen here just grazed by the knife. In front are seen the insertions of the sterno-cleido-mastoid just above the sternal notch.

Behind these two landmarks is seen the *thymus*, with its two lobes, and the large vessels of the neck on each side.

Behind the notch, between the two lobes of the thymus, is seen the trachea, its anterior curved portion of cartilage, the posterior soft collapsed portion. Posterior and to the left is seen the œsophagus. Posterior to the œsophagus is the cut body of the sixth cervical vertebra.

PLATE II.



Thymus.

PLATE III.

This section is made and situated as to the knife exactly as that of Plate II.

In front is seen the junction of the first rib with the sternum.

Behind the cut passes through the body of the first dorsal vertebra. Just behind the first piece of the sternum is seen the thymus (the darker portion) of an irregularly oval form.

To the right and behind the thymus is seen the collapsed innominate artery.

Behind the centre of the thymus is seen the trachea, behind and to the left of the trachea the œsophagus.

The apices of the lungs are seen on each side. Outside of the above district the heads of the humeri are seen, and behind obliquely stretching backward the scapulæ.

It is to be noted how the thymus occupies in this infant the greater portion of the superior opening of the thorax in the recent state.

PLATE III.

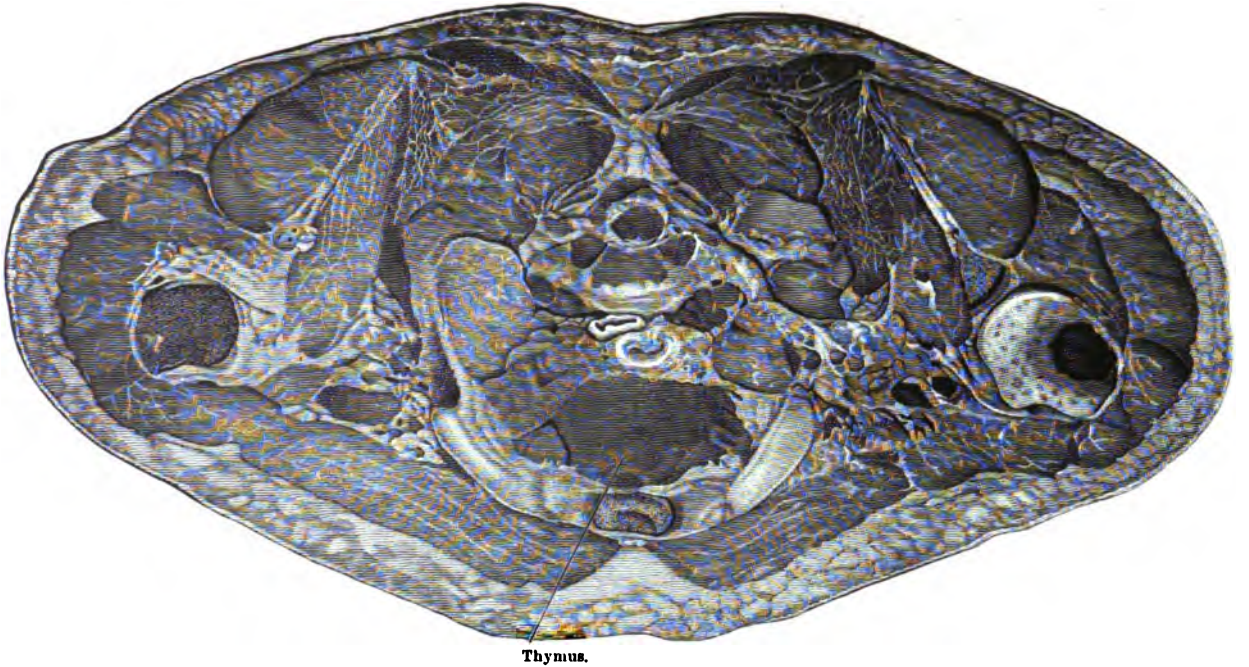


PLATE IV.

Behind is seen the body of the third dorsal vertebra. In front is seen the sternum at the level of the second rib.

The inferior surface of the section is shown in the plate, so that the parts are looked at from below.

The plate being held with the vertebra toward us, the right of the plate corresponds to the left of the child.

In front the large oval mass of the thymus is seen situated in the anterior mediastinum,

Posterior to this is the superior part of the aorta cut just below the points of origin of the innominate and carotid.

The œsophagus is situated to the right and behind the aorta.

The large extent of space occupied by the thymus is caused by its oblique position to the transverse direction of the cut.

PLATE IV.

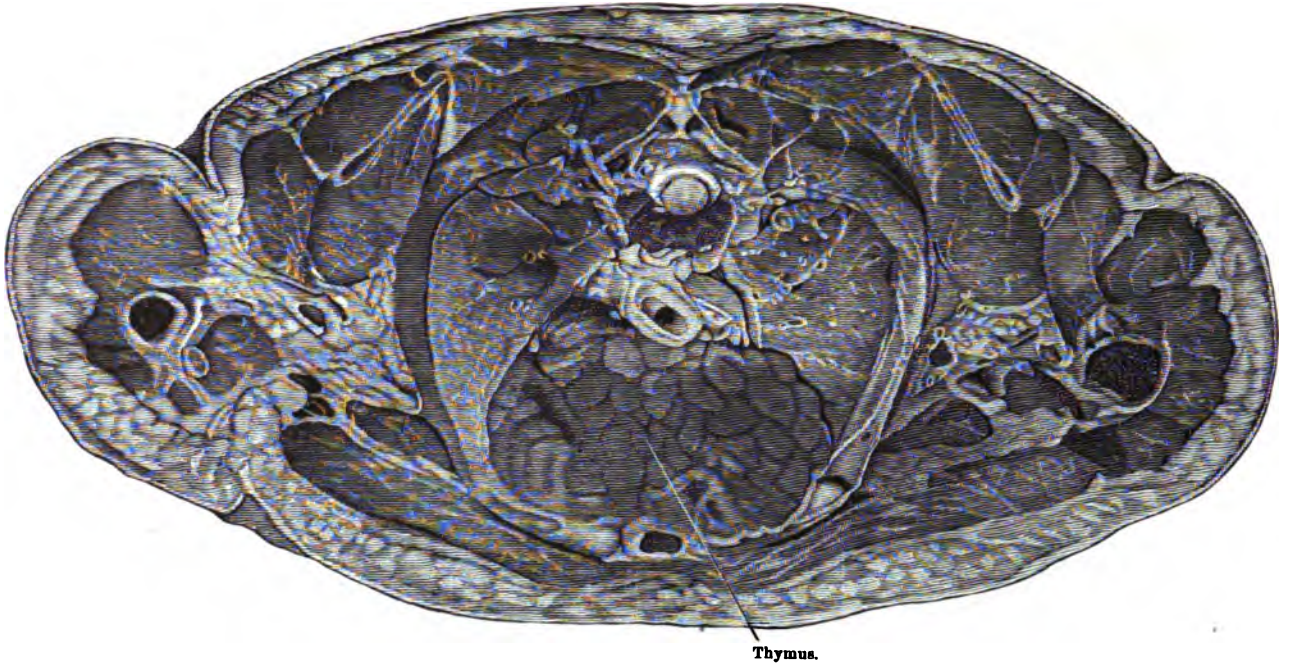


PLATE V.

Looking at the section from below upward. In front is seen the middle piece of the sternum. Behind, the body of the fourth dorsal vertebra. The ribs stretching between are indicated by the insertion of the intercostal muscles.

The section is cut on one side just beneath the axillary fold, on the other side it grazes this fold. The heart here forms a very good starting-point. The mass of the right ventricle is seen. The right and left auricles on each side above. The roots of the two lungs on each side behind the auricles. The unaërated lungs on each side. The œsophagus behind and to the right lying on the body of the vertebra. The inferior surface of the body of the vertebra is seen. The *thymus* is here shown as a pear-shaped body, the point of the pear stretching back. It is situated here between the heart and the anterior border of the right lung. In the specimen itself it lies on the anterior surface of the anterior layer of the pericardium. The peculiar shape here shown is caused by the oblique position of the heart and the thicker anterior border of the thymus.

PLATE V.

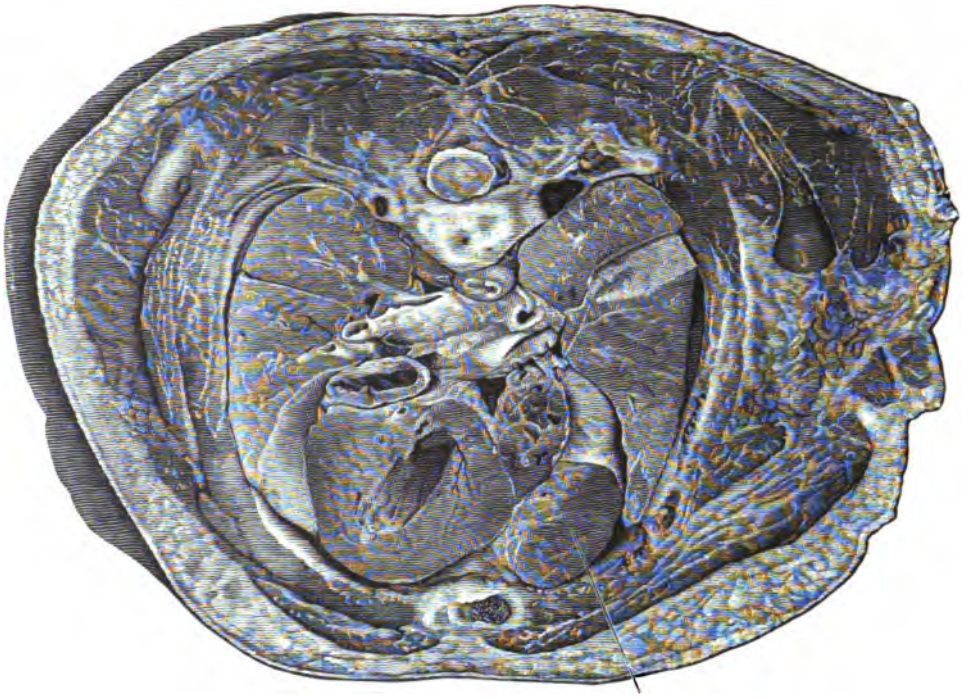


PLATE VI.

This section is the companion cut to the one of Plate V., fitting just below it. It was below the knife, while that of Plate V. was above the knife; we are here looking *down* on the section. Holding the plate, as in Plate I., the right of the plate corresponds to the right of the infant. Behind we see the body of the fifth dorsal vertebra. In front, the sternum.

The heart is seen surrounded by the pericardium, which is indicated by a rough line surrounding the heart. The right and left auricles are seen, as also the mass of the left ventricle, the walls of which are exposed to the left of the plate (holding the plate with the vertebra toward you). To the right are seen the walls of the right ventricle and part of the cavity of the same.

The *thymus* is seen to the right of the pericardium as a small, ragged oval piece of tissue, partly overlapped by the anterior border of the right lung. The unaërated lung with its fissures and roots is shown. The specimen having been prepared in strong alcohol, the shrinkage shows an exaggerated pleural space.

PLATE VI.

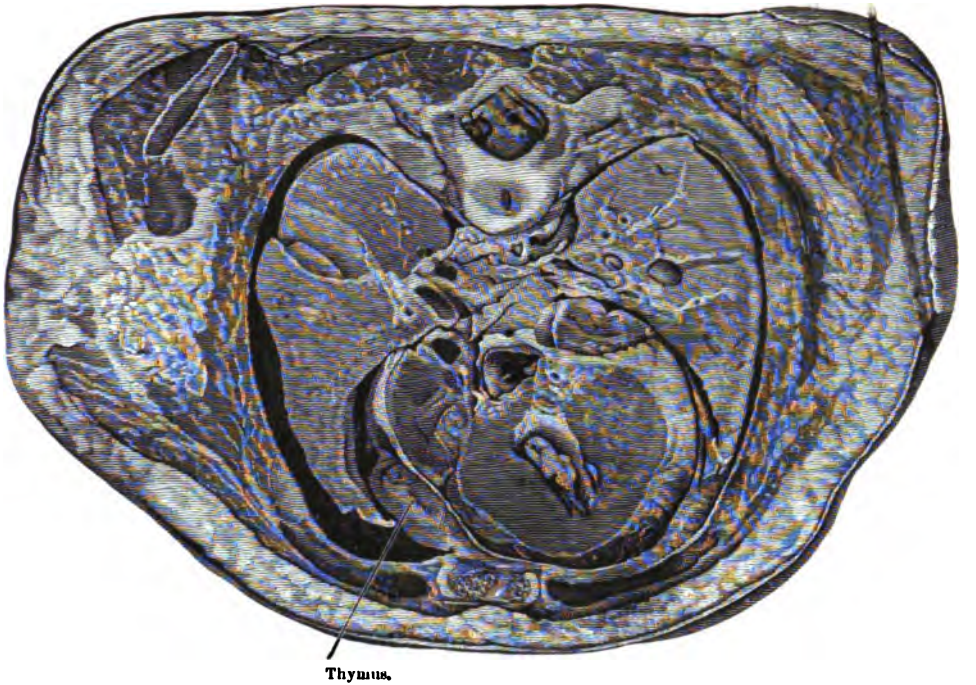


PLATE VII.

This section was taken from a child whose age, as near as could be calculated (post-mortem), was about two months. The cause of death was bronchopneumonia.

To understand this plate, we must imagine ourselves looking at the section from below upward with the vertebra of the subject toward us. The right of the plate corresponds to the left of the child.

In front is the top of the second piece of the sternum.

Behind is the inferior portion of the body of the *third* dorsal vertebra. The lungs are cut just above their roots.

The arch of the aorta occupies the centre of the plate.

Behind the aorta are seen the œsophagus and the right bronchus. *In front* of the aorta, occupying the anterior mediastinum and surrounded on the outside by a reflection of the pleura, is seen a triangular structure, the *thymus*.

This plate and accompanying mounted section show well the position of the thymus in the mediastinal space.

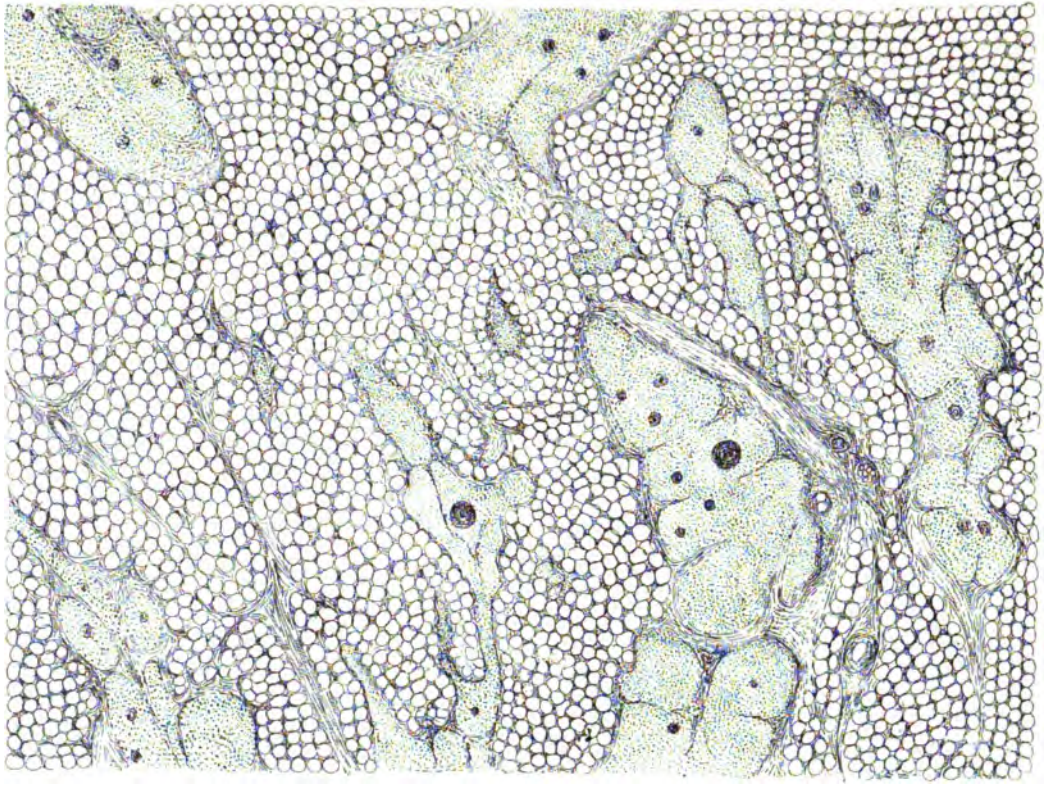
The mounted hand specimen belonging to the above plate shows a view of the superior surface of this section. Here the position of the thymus to the arch of the aorta and the points of origin of the great vessels of the neck, and the very close relation to the trachea, are well shown.

PLATE VII.

PLATE VIII.

Adult persistent thymus. (See p. 305.)

PLATE VIII.



TUBERCULOSIS OF THE THYMUS.

All the plates are drawn accurately from the actual specimens by means of the Abbé camera lucida.

PLATE IX

is a section of a portion of the thymus of Case I. The drawing is simply topographical, and is intended to show the manner of eruption of the miliary tubercles in the acini of the thymus. The miliary tubercles are represented in the upper part of the picture as irregularly round bodies with lighter centres (cheesy) in the midst of the thymus tissue. The normal acini are represented with a darker external zone (cortical), and a lighter central zone (medullary portion).

PLATE IX.

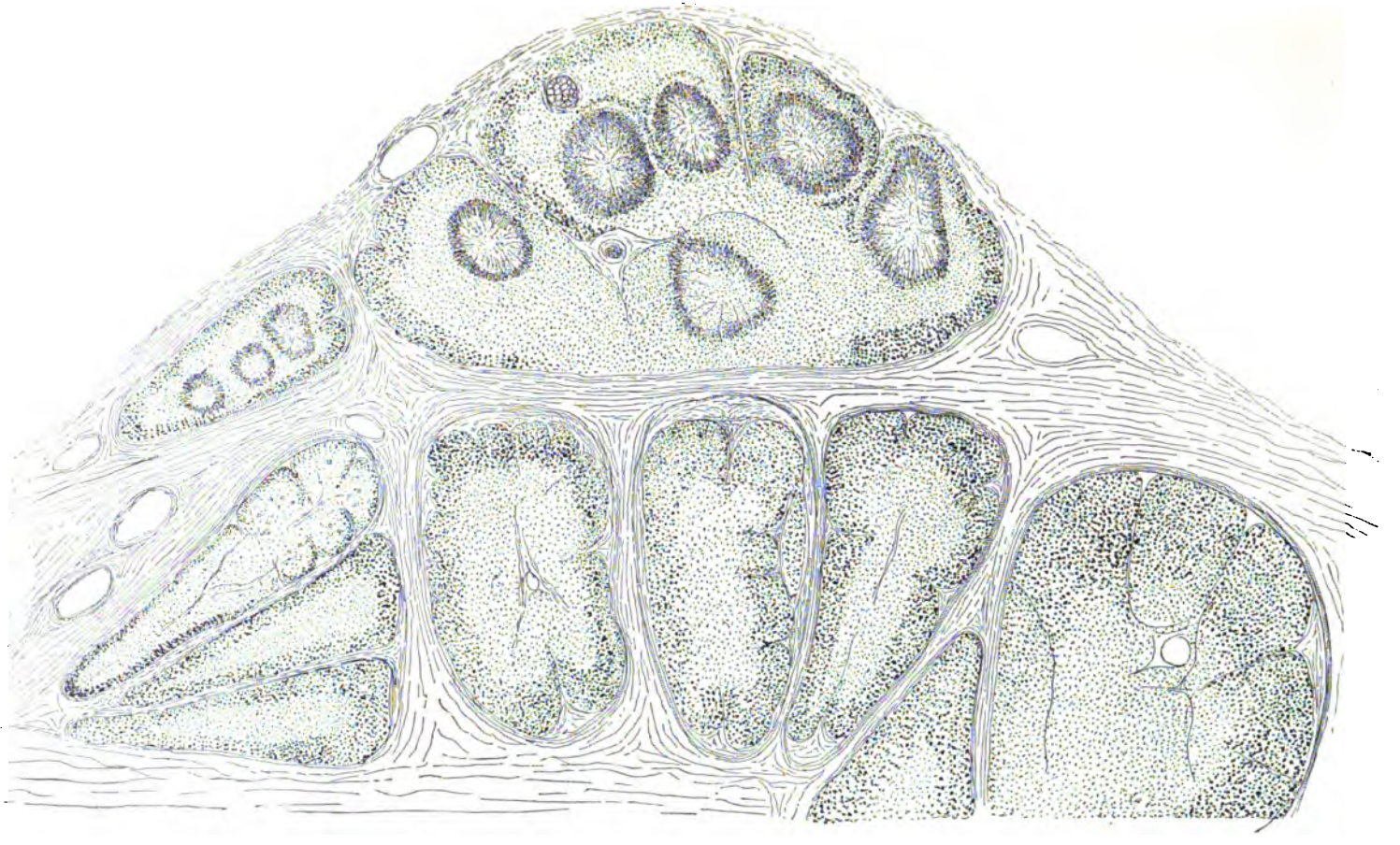


PLATE X. ($\times$ 185 diameters)

is also taken from Case I. It is intended to show more closely the structure of the miliary tubercle, its periphery and cheesy centre. The tissue between is represented by closely packed round cells, rich in bloodvessels, which are naturally injected with blood. The reticulated structure of the periphery of the basement-substance of these miliary substances is shown here as small spaces, from which the round and polygonal cells have been shaken by manipulation.

PLATE X.

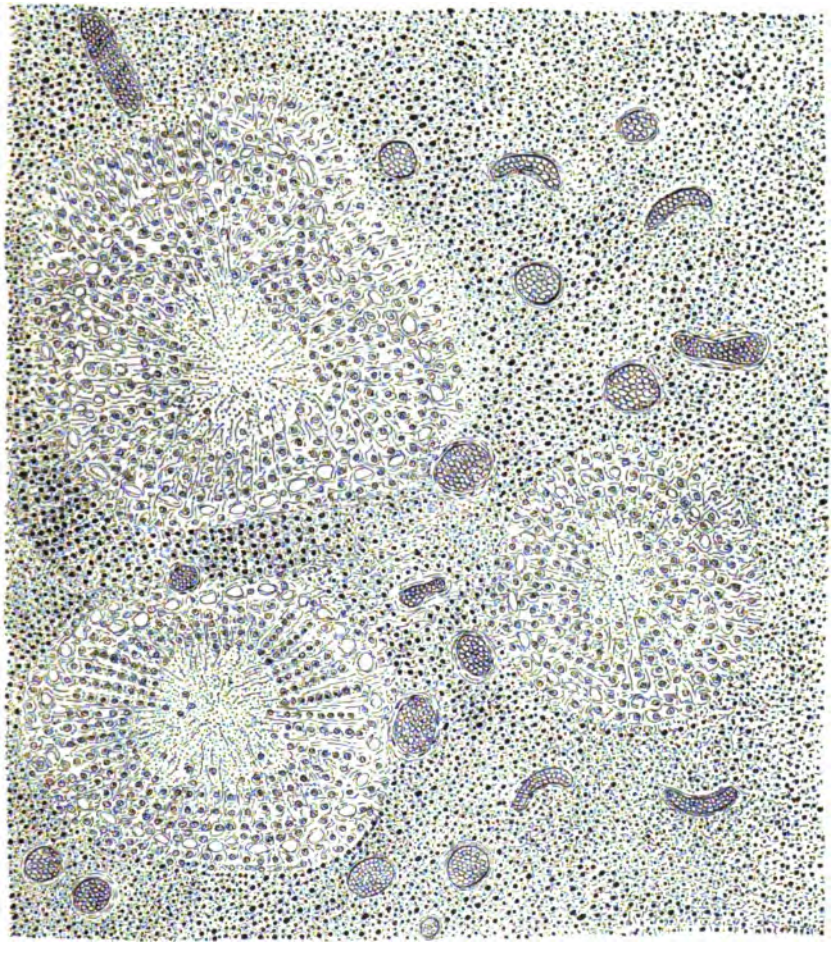


PLATE XI. (× 85 diameters)

is the periphery and central portion of coagulation-necrosis of a cheesy nodule of the thymus; here the tubercle granula, unlike the *previous specimen plate*, show giant cells. Some of the granula have cheesy centres; the spaces between the granula are filled with small round cells.

PLATE XI.

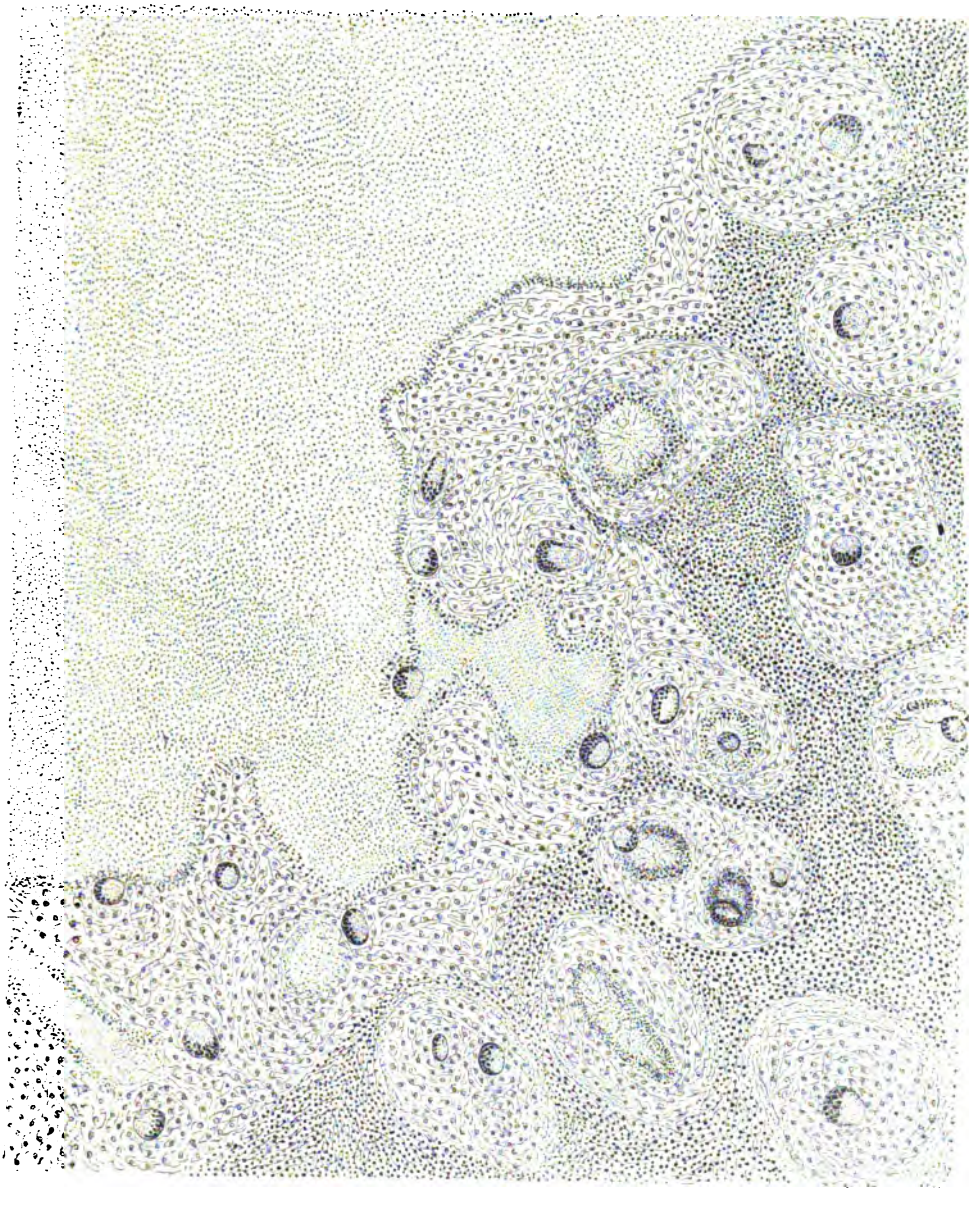
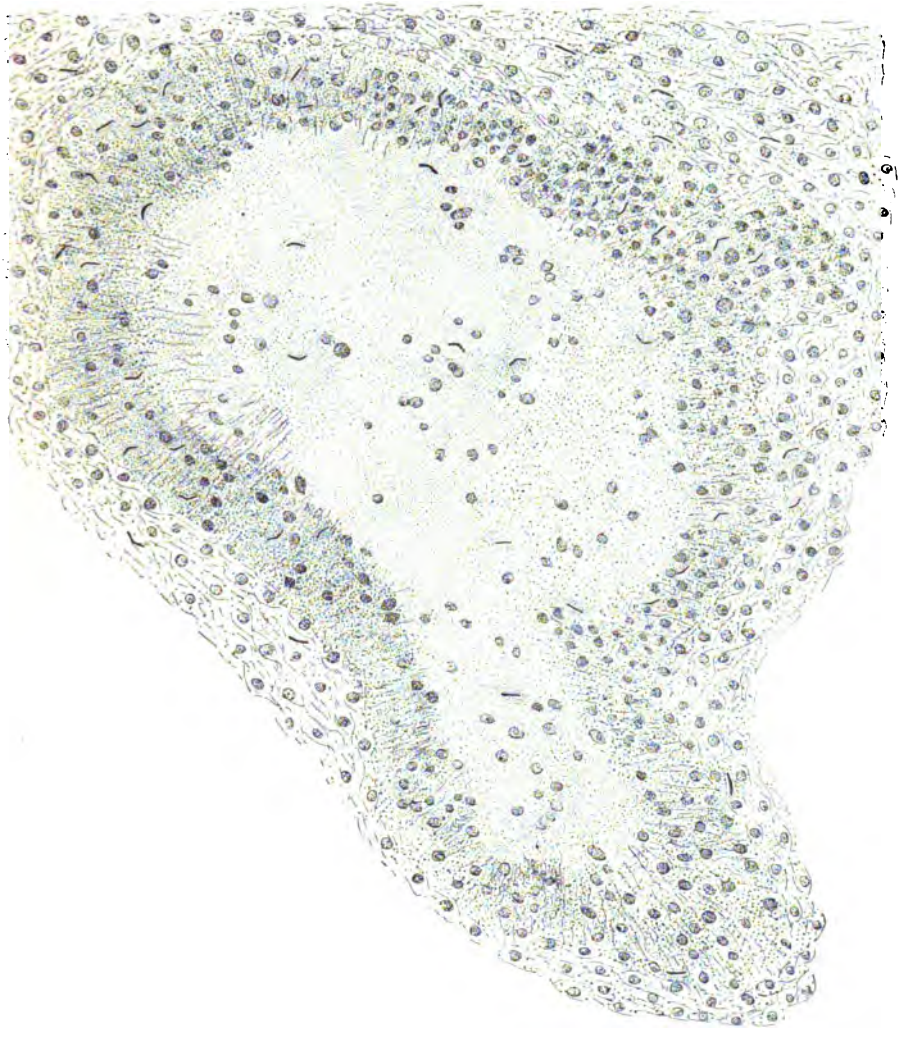


PLATE XII. (× 550 diameters. *Tubercle bacilli*)

shows under a high power the appearance of a miliary tubercle of Case I.

The specimen prepared by Koch-Ehrlich method to show tubercle bacilli. The miliary tubercle (decolorized) was first drawn with the camera lucida, the tubercle bacilli were counted with an immersion $\frac{1}{12}$, and then as near as possible drawn into the sketch. The tubercle bacilli are, therefore, a little exaggerated as to their actual size and thickness for the sake of illustration. They are seen in *moderate numbers only*, and are found in the cheesy centre, in the periphery of the miliary tubercle and in the tissue between the miliary tubercles.

PLATE XII.

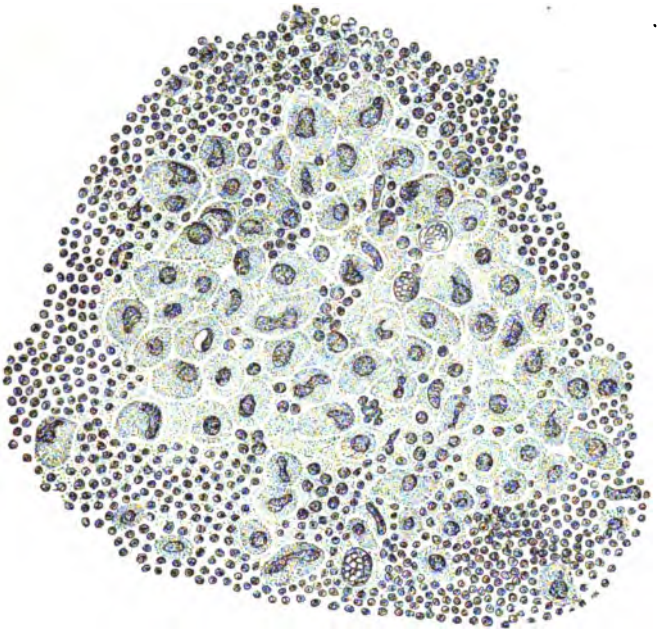


THYMUS IN DIPHTHERIA.

PLATE XIII.

Thymus from a severe case of diphtheria.

PLATE XIII.



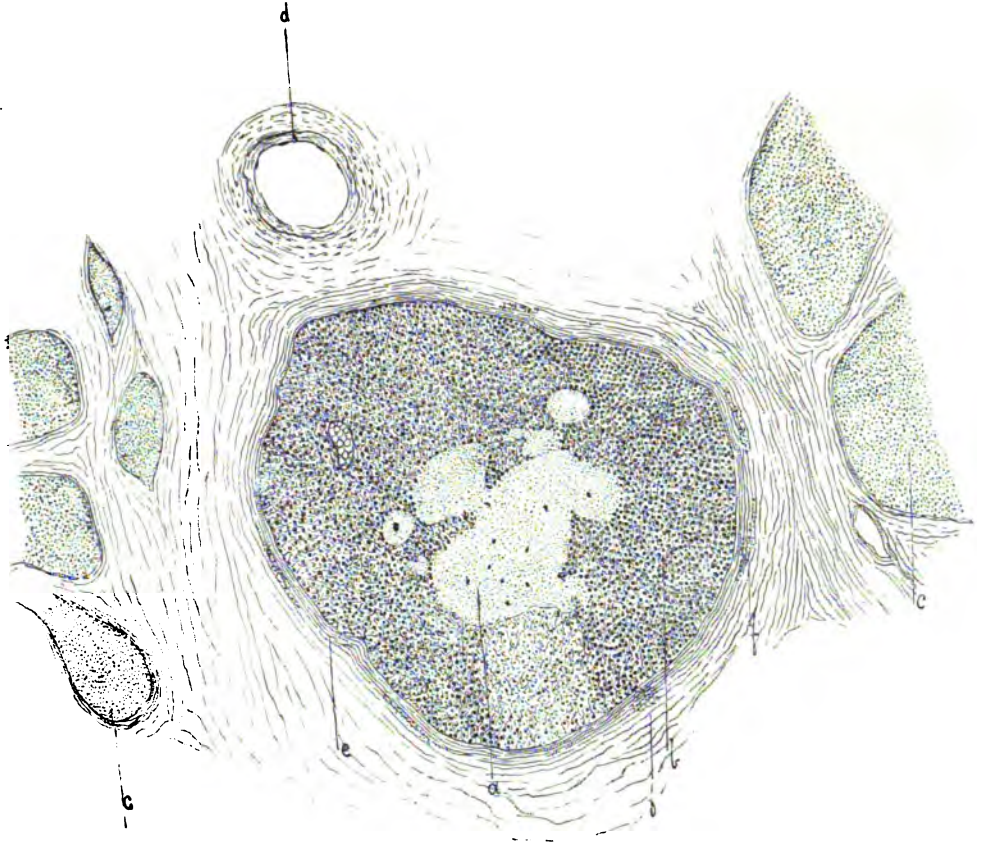
SYPHILIS OF THE THYMUS.

PLATE XIV.

Gumma (?) in the thymus.

a. Cheesy centre; *b*. Small round cells; *c*. Thymus tissue; *d*. Bloodvessel; *e*. Connective tissue zone; *f*. round-cell infiltration.

PLATE XIV.



RECENT RESEARCHES RELATING TO THE ETIOLOGY OF YELLOW FEVER.

By GEORGE M. STERNBERG, M.D.,
MAJOR AND SURGEON UNITED STATES ARMY.

At the close of an address, delivered before the College of Physicians of Philadelphia, in April last (published in *The Medical News* of April 28, 1888), the writer remarked as follows:

From what has been said, it will be seen that the question of yellow fever etiology is still unsettled, and that it remains, in fact, just where it was left by the Commission sent to Havana, in 1879, by the National Board of Health. The researches made by myself, as a member of that Commission, showed that in yellow fever there is no microörganism present in the blood of the sick demonstrable by the highest power of the microscope. This conclusion is supported by my more recent researches by the method of cultivation. Before, however, abandoning all hope of finding a specific "microbe" in the tissues I desire to make cultures from the various organs obtained at the earliest possible moment after death. Unfortunately, the opportunity did not offer itself for this important experiment during my visit to Brazil and to Mexico last year, but I hope to have the desired opportunity in Havana, during the present summer, and shall also give special attention to a search for the yellow fever germ in the alimentary canal, in which it may possibly be located, as is the case in cholera.

A week after the above was written, I proceeded to Havana for the purpose indicated, and during my stay in that city was able to make ten autopsies in typical cases of yellow fever occurring in the Military Hospital. Most of these autopsies were made within three hours after death, and the material secured should contain the specific infectious agent in the disease under investigation if this is present in yellow fever cadavers. The study of this material, and especially of the microörganisms obtained from the alimentary canal, has occupied me continuously up to the present time, and I am still far from having completed my investigations, the details of which will be given in my

final report to the President of the United States. I have thought, however, that it might be well to avail myself of the present occasion to give an account of some of the microörganisms encountered, and especially of that one—my bacillus *a*—which I found to be abundantly and constantly present in material obtained from the stomach and small intestine of these yellow fever cases, and which was also the microörganism most frequently encountered in my cultures from the liver and kidney. I can best give the facts relating to the presence of this bacillus by quoting from an unpublished report submitted to the President of the United States, in compliance with my instructions, immediately after my return from Cuba:

"*Blood.*—In eight of the ten cases my cultures from blood drawn from the heart have remained sterile. This result fully confirms the conclusions heretofore reached by the method of direct examination and cultivation of blood drawn from the fingers of patients during life.

"In the two exceptional cases a few colonies appeared; in one, my bacillus *a*, to be described hereafter; in the other, bacillus *a* and a bacillus resembling it in its morphology, but shown to be different by the appearance of its colonies in gelatine.

"*Urine.*—In seven cases my cultures from urine drawn through the walls of the bladder have remained sterile; in three cases microörganisms were present. In one, bacillus *a* associated with two or more different bacilli; in one, a liquefying bacillus (*e*) which forms a green pigment; in one, a bacillus resembling *a*, but not identical with it.

"*Kidney.*—In six of the ten cases I have obtained microörganisms from the kidney, and in four cases my cultures remained sterile. In four cases my bacillus *a* was present associated with one or more different bacilli; in one case a green liquefying bacillus, and in one *B. termo* was the organism encountered.

"*Liver.*—In seven cases my cultures from the liver have remained sterile, and in three they have given me colonies: bacillus *a* in two, and *B. termo* in one.

"*Stomach and Intestine.*—As was to have been expected, I have encountered a variety of microörganisms in my cultures from material obtained post-mortem from the stomach and intestine. . . . . .

"I have encountered bacillus *a* in eight out of ten cases in my cultures from the alimentary canal, and if it turns out on fuller investigation that it is identical with bacillus *f*, which was the prominent form in my last two cases, it will be the one organism which is uniformly and abundantly present, and consequently the one which seems most likely to bear an etiological relation to the disease. . . . . .

"In two cases a liquefying bacillus which produces a green pigment has been prominently present in my cultures. A complete analysis of all the organisms present in the stomach and intestine in these ten cases was quite

impossible in the time at my command, and I have naturally given the most attention to the bacillus which was most frequently and abundantly present. In my first six cases I used agar-agar as a culture medium, and having found that my bacillus *a* would grow in an acid medium, Dr. Finlay, at my request, prepared for me agar-agar which was slightly acid. In my most recent cases I used again neutral agar or flesh-peptone-gelatine. It is probable that I would have recognized the presence of liquefying bacilli more frequently if I had used gelatine for my cultures throughout.

"The material which has served for my cultures from the intestine has usually been obtained from the ileum, where the characteristic black matter is the most abundant, sometimes as a fluid filling the lumen of the intestine, but more frequently as a viscid material from admixture with mucus, which adheres to its walls." . . . .

"Bacillus *a* is from one and one-half to four times as long as broad, and has a diameter of about 0.6 of a micromillimetre; the dimensions vary considerably, however, in different cultures, and even in the same culture. Under certain circumstances the ends of the rods are seen to be more deeply stained than the central portion. This appearance is not, however, constant, and seems to depend upon the age of the culture, and upon the time during which it is subjected to the action of the staining reagent. The ends of the rods are more or less rounded, and they are often associated in pairs.

"Young colonies in gelatine (Esmarch tubes) have a pale straw color which afterward becomes pale brown; they are spherical and homogeneous, or lobate. Colonies which have come to the surface often present the appearance of a rosette, or of a flower with its petals expanded, as in the daisy. From cultures in which lobate colonies and rosettes were the prevailing form I have obtained in a second series of Esmarch tubes, made from a single typical rosette, colonies which were not lobate, and which did not form rosettes (spherical straw-colored homogeneous colonies); and in these cultures superficial colonies when young have the appearance of a fragment of broken glass; later they present an irregular margin, and a more or less wrinkled appearance of the interior. These are the characters of bacillus *f* found as the prevailing form in my cultures from the stomach and intestine in my last two cases.

"It is a very curious fact that during the last week of my stay in Havana I failed entirely to obtain my bacillus *a* in lobate colonies and rosettes, although I have three times repeated the experiment of making a series of Esmarch tubes from a single typical colony in form of rosette in Esmarch tubes preserved from my earlier cases. In every instance I have had instead colonies which are identical with those in other Esmarch tubes inoculated at the same time from single colonies of my bacillus *f* of Case 9 and Case 10. As the morphology of the bacilli is also the same I am forced to the conclusion that bacillus *a* and bacillus *f* are one and the same, and that the character which during my earlier experiments I found to be constant—*i. e.*, the colonies in form of a rosette—is not so, but that under certain circumstances, at present undetermined, this bacillus forms lobate colonies and rosettes, and

under other conditions homogeneous spherical colonies in the deeper portion of the gelatine, and broken-glass colonies upon the surface. Later these colonies have an irregular vitreous-looking margin, and the interior has a brown color, more or less intense. These are the characters given by Flügge of gelatine colonies of the bacillus of Esmarch, discovered in cholera cadavers at Naples in 1884, and the morphology of this bacillus as given by Flügge resembles that of the bacillus which I have found as the prominent form present in the intestine of yellow fever cadavers in Havana. The question of identity I must leave unsettled until I have an opportunity to study the two microörganisms side by side."

Since my return to Baltimore I have given much time to the elucidation of the question referred to. It is evident that if identity is once established with any known bacillus found elsewhere than in connection with yellow fever the microörganism can be excluded as the possible specific cause of this disease. But the present status of bacteriology makes it also evident that great care must be taken in assuming the identity of microörganisms from different sources; and that morphological characters, or even similarity of growth in culture media, will not alone suffice. Any constant difference in physiological characters, as, for example, in pathogenic power when tested on various animals, must be accepted as establishing a specific form, or a permanent variety, which amounts to the same thing. We now know, for example, two spirilla which resemble closely the spirillum of Asiatic cholera (the Finkler-Prior spirillum and the "cheese spirillum" of Deneke), but which have, nevertheless, been shown by carefully conducted experiments to be different, in certain particulars which would easily escape notice upon a superficial comparison.

I have felt that with this knowledge to guide me I could not lightly exclude any microörganism as common, and especially that a bacillus which was so prominent by reason of its constant and abundant presence should receive careful consideration.

Two bacilli have been described, both of which are found in the intestines of healthy individuals, which in their morphology resemble my bacillus *a* found in the alimentary canal of yellow fever cases. These are the *Bacillus Neapolitanus* of Emmerich, and the *Bacterium coli commune* of Escherich. Both of these microörganisms have been the object of extended studies by German bacteriologists, and especially the bacillus of Emmerich, on account of his claim that it is the specific etiological agent in the disease with which he found it

associated. This claim has not been substantiated; on the other hand, it has been shown by Weisser[1] that "in human feces, normal as well as abnormal, in the air, and in putrid material bacteria are found which in their morphology, their biological functions, and their pathogenic action upon animals are identical with the so-called Naples cholera-bacterium of Emmerich."

I have had for comparison a culture of the Emmerich bacillus, originally from one of the bacteriological laboratories of Germany, preserved in the collection maintained under the direction of Prof. William H. Welch, in the pathological laboratory of Johns Hopkins University. For a culture of the Bacterium coli commune I am indebted to Dr. William D. Booker, of Baltimore, who had it directly from Escherich. The characters of both of these microörganisms have been carefully defined, but I find no account of the lobate colonies (deep), or of the superficial colonies in the form of rosettes which became for me, during my earlier experiments in Havana, the diagnostic character by which I recognized my bacillus *a* in gelatine Esmarch tubes. By lobate colonies I mean that, instead of being spherical and homogeneous, the colonies are divided into a small number of distinct masses, without losing their spherical outline. In some cases this apparent segmentation of the colonies extended further, and they became coarsely granular in appearance. The superficial colonies in the form of rosettes also varied considerably, the margins being sometimes made up of symmetrical lobes shaped like the petals of a daisy—Marguerite—and at others being more deeply cleft and dentate. If this character of growth had proved to be constant, it would suffice to differentiate my bacillus *a* from the two microörganisms in question, but it proves not to be constant, and since my return to Baltimore I have only obtained these rosette colonies exceptionally, and have not been able to determine the precise conditions which determine their formation. But I have ascertained that, cultivated side by side in the same media and under the same conditions of temperature, the colonies formed in gelatine Esmarch tubes, are identical in the case of my bacillus *a* and the colon bacillus of Escherich. The growth upon agar-agar and upon potato is also identical, and the reducing power upon litmus added to sterilized milk.

[1] Ueber die Emmerichschen sogenannten Neapler cholera bacterien. Zeitschrift für Hygiene, Band 1.

My gelatine cultures have been made in flesh-peptone-gelatine containing twenty per cent. of gelatine, instead of ten per cent. as in Koch's original formula. I have found this to stand the summer heat of Baltimore without liquefaction, and during the months of May and June used it with great satisfaction in Havana, for Esmarch tubes and stick-cultures. It will stand a temperature of about 86° F. (30° C.), and, so far as I can see, answers as a culture medium for diagnostic purposes quite as well as that containing ten per cent. of gelatine.

The idea which I had entertained that isolated colonies in Esmarch tubes should give me pure cultures of a single organism, has proved not to be true in most instances in my first series of tubes inoculated with material from the intestine or stomach, and it seems probable that such colonies commonly originate from little masses of associated microörganisms rather than from a single bacillus. In making gelatine stick-cultures in twenty per cent. gelatine from single Esmarch colonies of my bacillus *a* I have frequently had, both in Havana and since my return to Baltimore, an outgrowth at intervals along the line of puncture which is shown in Fig. 1 of the plate accompanying this paper. This consists of feathery tufts growing out into the gelatine. To test the question whether these tufts represent a different microörganism, or a different mode of growth of the same, I have several times carefully broken the test-tube containing a growth like that shown in Fig. 2, and have made a stick-culture in gelatine from the feathery tuft and also from that part of the stick which was free from these outgrowths: one, from the tuft, has been a similar growth all along the line of puncture, as shown in Fig. 3; the other has resembled precisely that portion of the original stick culture which was free from these outgrowths. Having, in Havana, made this differentiation, I designated the tufted growth bacillus *l*. The morphology of the bacillus when cultivated in bouillon or obtained from the surface of an agar or gelatine culture appeared to be identical with that of bacillus *a*; but preparations mounted directly from the feathery tufts showed that, together with short oval bacilli of the typical form, there were numerous long filaments like that shown at *a* in Fig. 6. A very curious thing about my stick-cultures of this bacillus *l*, made in Havana, was that after a few days they assumed a brownish color, which in time became black.

In cultivating Emmerich's bacillus in twenty per cent. gelatine, at

a temperature of 28° to 30° C. (82.4° to 86° F.), I have obtained the same radiating feathery growth as is presented by my bacillus *l*. This is shown in Fig. 4, which, like the other figures illustrating this paper, is copied from a photograph made by myself. This mode of growth, so far as I know, has not been described, but I have no reason to doubt the authenticity of the stock from which the culture was made. The growth upon potato and the morphology correspond with the descriptions of those who have specially studied this bacillus and with my bacillus *l*. I am, therefore, forced to the conclusion that they are identical. The morphological characters of this bacillus are shown in Fig. 6, which is made from a bouillon culture of bacillus *l*, separated, as heretofore stated, while in Havana, from a stick-culture of my bacillus *a* (Bacterium coli commune of Escherich).

I must now call attention to Fig. 5. This is from a stick-culture in twenty per cent. gelatine of the colon bacillus of Escherich, obtained from him, as heretofore stated, through Dr. Booker, of Baltimore. We have here a feathery outgrowth along the line of puncture which appears to be identical with that shown in Fig. 1, from my bacillus *a*. To test the question of identity, I broke this tube and made a stick-culture in twenty per cent. gelatine from the tuft at *a*, and another from the clear portion of the column above this point. The result was a growth in the first tube like that from which the inoculation was made—similar to Figs. 3 and 4, and in the second tube the characteristic growth of the colon bacillus, viz., spherical, light brown colonies, crowded together into a central core at the upper part of the puncture, but more or less distinct and separate at the lower extremity. Thus it will be seen I have had the same result from this authentic culture of the colon bacillus, which was sent by Escherich to Dr. Booker and had been passed through a series of Esmarch tubes by the last-named bacteriologist, as that obtained in Havana, and since in Baltimore from stick-cultures of my bacillus *a*.[1] This forces me to one of two conclusions, either the colon bacillus of Escherich and the bacillus of Emmerich may remain associated after passing a culture containing them through a succession of plate cultures (Esmarch tubes), or the so-called bacillus of Emmerich (my bacillus *l*) is a variety of the colon bacillus—a sport which is pro-

[1] I have since obtained the same result from cultures made in Decatur, Ala., from the feces of yellow fever cases.

duced under certain circumstances, and which retains its distinctive characters for a certain time. I am not prepared at present to decide this question in a definite manner, but propose to keep it in view in my future studies of these microörganisms.

While in Havana and since my return to Baltimore, I have made numerous inoculation experiments, in rabbits and guinea-pigs, with cultures of this bacillus. The general results obtained may be briefly stated as follows:

The injection into the cavity of the abdomen of a considerable quantity of a bouillon culture—for a full-grown rabbit from 3 to 5 c. c. —causes a decided elevation of temperature which may amount to more than two degrees Centigrade (3.6° F.), and death usually occurs within two to four days. A certain proportion of the animals, however, survive these large doses, and in smaller amounts (less than 2 c. c.) recovery is the rule. My experiments show that animals which survive such an inoculation are subsequently less susceptible to the action of the fever-producing ptomaine contained in the cultures. That the febrile movement is due to a ptomaine produced during the active development of the microörganism is indicated by the fact that cultures sterilized by heat (70° C. (158° F.) for ten minutes) likewise give rise to a temporary rise of temperature.

In fatal cases, in which non-sterilized cultures were injected into the cavity of the abdomen, I have recovered the bacillus from the blood and tissues, in pure cultures, and these possess pathogenic properties identical with those shown by the original cultures.

The most striking post-mortem appearance in animals which succumb to an intra-peritoneal injection is a rosy hyperæmia of the small intestine. The liver is dark in color, full of blood, and rather soft. The spleen is normal in appearance.

These results show that in its pathogenic properties, as well as in its morphology and growth in culture media, my bacillus *a* corresponds with the *Bacterium coli commune* of Escherich and consequently that it is not the specific etiological agent in yellow fever.

The facts stated indicate that my bacillus *l* is identical with the bacillus of Emmerich and that this is a variety, or "wuchsform" of the colon bacillus of Escherich.

In the course of my researches I have isolated various other microorganisms from material obtained from yellow fever cadavers. But,

Illustrations for Dr. Sternberg's Article:

RECENT RESEARCHES RELATING TO THE ETIOLOGY OF YELLOW FEVER.

Fig. 1

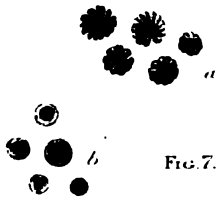


Fig. 7.

Fig. 6.

Fig. 2.

Fig. 4.

Fig. 3

Fig. 5.

as no one of these has been found constantly, or, when tested upon animals, has caused morbid phenomena similar to those of yellow fever, and as my researches are still in progress, I shall not in the present paper give an account of the characters of these various micro-organisms, which it is my intention to describe in detail in my final report to the President of the United States.

My conclusion is briefly stated, viz.: The specific infectious agent in yellow fever has not yet been demonstrated.

EXPLANATION OF PLATE.

FIG. 1.—Bacillus *a*; "stick-culture" in flesh-peptone-gelatine (20 per cent. of gelatine); from a photograph.

FIG. 2.—Portion of same stick-culture shown in Fig. 1, magnified about 4 diameters; from a photograph.

FIG. 3.—Bacillus *l*; a gelatine stick-culture obtained from the feathery outgrowths shown in Figs. 1 and 2 (20 per cent. of gelatine).

FIG. 4.—Emmerich's bacillus cultivated in 20 per cent. gelatine, at 27° C. (80.6° F.).

FIG. 5.—Bacterium coli commune of Escherich, cultivated in 20 per cent. gelatine, at 27° C. (80.6° F.).

FIG. 6.—Bacillus *l*; magnified 1250 diameters; from a photo-micrograph by Dr. Sternberg.

FIG. 7.—Colonies of bacillus *a* in flesh-peptone-gelatine; *a* superficial colonies in rosettes; *b* deep colonies, usual form; *c* superficial colonies, usual form.

PULSATING PLEURISY.

By WILLIAM OSLER, M.D.,
PROFESSOR OF CLINICAL MEDICINE IN THE UNIVERSITY OF PENNSYLVANIA.

PULSATING pleurisy is such a rare condition that the following case is worth placing upon record.

Strain in lifting: pain in left side; rapid effusion, at first serous, necessitating two aspirations; pyo-pneumothorax; pulsation of side; free drainage; recovery. —James F., aged twenty-three years, laborer, was admitted to the University Hospital March 3, 1888. Family history good; none of his relatives has had phthisis; with the exception of an attack of rheumatism in 1885, he has enjoyed uniformly good health.

On February the 23d, eight days before admission, he sprained his back by lifting a piece of timber twenty feet in length and ten inches in diameter. Three men were lifting it, but one of them let go his hold, so that the patient had to exert his utmost strength to support his end of the piece. At the time he did not feel any discomfort, but that evening he became sore and stiff. He slept well, and the next day, a public holiday, he went about with his comrades, but complained on several occasions that he had sprained his back with heavy lifting. During the evening the pain grew worse and he passed a restless night. The following morning he did not feel well enough to get up and the pain became almost unbearable. There was no cough or shortness of breath; he does not know whether he had any fever.

From the 26th to the 3d, the date of his admission, he was "up and down;" in bed part of the time, and part of the time by the kitchen fire. The pain in the back was his only complaint. He is positive he had no cough, but he was short of breath.

On admission, the patient looked very ill; face flushed, tongue dry and coated; respirations 36, temperature 104⅘°. He was able to lie down in bed. The importance attached to the lifting on February 23d may be gathered from the fact that he was admitted to the surgical ward as a case of injury to the back, and subsequently transferred. The day after admission he had much pain, of a cutting nature, in the left side, in the axillary region outside the nipple. There was also extreme tenderness on palpation.

I saw the patient for the first time on March 5th, and was struck with his distressed appearance. He lay propped up in bed, had slight dyspnœa, dry

tongue, pulse 100, temperature 100°. He complained of severe pain in the left back, just below the scapula. On examination the existence of a large effusion in the left pleura was readily determined. The cardiac impulse was in the third right interspace in the para-sternal line. I noticed one or two special features in the case: the sudden onset after the strain, the remarkable rapidity with which the effusion had taken place, and the unusual amount of pain on palpation.

March 6th, a more thorough examination was made: expansion was almost negative on the left side; the intercostal spaces indistinguishable. The only cardiac impulse visible was in the third right interspace. Tactile fremitus was diminished. There was uniform dulness on the left side reaching to the clavicle and extending a little beyond the right margin of the sternum in the second interspace. By a hypodermic puncture the fluid was shown to be serous. On the principle that a full pleura demands immediate aspiration this operation was performed by Dr. Daland before the ward class, and fifty ounces of clear serum removed.

On the 5th and 6th the patient seemed much relieved. By the 9th the fluid had reaccumulated and the heart beat was again visible to the right of the sternum, the dulness reaching to the level of the clavicle. Aspiration was again performed and two quarts of slightly turbid serum removed. He seemed to improve very much after this. The temperature kept below 99°, the dyspnœa was relieved, the appetite improved, and with the exception of pain in the left side he felt very comfortable. The signs of effusion persisted and the heart beat could be seen in the third left and the third right interspaces.

16th. The following note was made: Condition of patient improved, pulse 84, respiration 28, temperature 98°; dyspnœa seems quite relieved, sleeps with his head low, complains only of chest pain; left side scarcely moves, and looks somewhat bulged. Cardiac impulse seen in the second and third spaces on both sides close to the sternum. The left interspaces not specially prominent. Tactile fremitus felt at the extreme apex, but nowhere else on the left side; percussion note clear to the second rib, dull below this. On the right side normal.

Auscultation: Inspiration is loud and breezy in the subclavicular and supra-scapular regions. Below these points the respiratory murmur has a distinctly amphoric character, contrasting remarkably with the breath sounds over the upper part of the lung. The voice sounds in the lower regions are very articulate; no metallic tinkling on coughing. No bell sound, nor could succussion be obtained.

19th. To-day, for the first time, it was observed that the percussion note in the left subclavicular region was distinctly tympanitic. Dulness extended from the upper border of the third rib, and was distinctly movable. This was very marked when he lay on his right side, in which position the percussion note in the axilla became hyper-resonant without tympanitic quality. Posteriorly in erect position at outer angle of the scapula the percussion note was

distinctly tympanitic. To-day, for the first time, the bell sound was obtained with the coins.

26*th*. Since the 20th he has had irregular fever reaching 102° in the evening. He has, however, felt much better. The physical signs persist; in the recumbent posture the note is hyper-resonant to the lower border of the third rib. Just below the nipple it is distinctly tympanitic, from this point into the axilla there is dulness. When he turns on his right side the entire left axilla is resonant. There is amphoric breathing from the third rib, best marked in the lower axilla where the percussion note is dull. There was noticed to-day in the fourth, fifth, and sixth interspaces in the mid-axillary line a remarkable pulsation. The whole side received a very positive shock, systolic in time and synchronous with the cardiac impulse in the third right interspace just above the nipple; the hand placed on the left side is distinctly lifted with each impulse. When he lies toward the right side the pulsation in the left axilla is a little more marked than when he is flat on his back.

27*th*. The common decubitus is on the left side and in this position the heart impulse is well seen just above the right nipple. The pulsation in the left mammary and axillary regions is very marked. The maximum intensity is outside the left nipple. When he turns on the right side the pulsation is most marked in the fifth and sixth interspaces in the mid-axillary line. Palpation gives a very decided heave and a distinct shock is felt. In the erect posture, the pulsation is not so forcible, though still very evident. The coin sounds are now unusually distinct. Succussion is not attainable.

29*th*. The left chest looks larger and fuller than the right; it is completely immobile. The intercostal spaces are obliterated with the exception of the sixth, which is still very visible. The systolic impulse on the left side is very marked, and can readily be seen by the students in the distant seats of the amphitheatre. Measurement on the right side gives sixteen and one-eighth inches, expansion one-half inch; on the left side, seventeen and one-quarter inches, practically no expansion.

Palpation.—Tactile fremitus is absent on the left side. Heaving impulse in mammary and axillary regions well felt with the hand.

Percussion.—Clear, hyper-resonant note to upper border of the third rib. From the third to the fifth it is distinctly tympanitic. Below this, in the axillary region there is dulness. When he turns on his right side the pulsation in the mammary and axillary regions is more marked. Where the percussion note was dull, it is now tympanitic.

Auscultation.—In the left infra-clavicular region the breath sounds are loud and distinct, not amphoric. In the third and fourth interspaces the respiratory murmur is scarcely audible. In the axillary regions there is distant but distinct amphoric breathing, very clearly heard when a deep breath is taken. No special amphoric echo about the voice, the vibrations of which are not communicated to the ear; posteriorly there is distant amphoric breathing.

31*st*. The irregular fever has persisted and the presence of pus was demonstrated with a hypodermic needle. It was decided to open the pleura, which was done by Dr. Ashhurst; fully three pints of pus escaped. A large drainage

tube was inserted in the eighth interspace, below the angle of the scapula. After the operation, the heart did not return to the normal position, though it beat to the left of the sternum. Patient stood the operation very well, the evening temperature was only 98.2°.

April 3. Pulse 92, respiration 24, temperature 98°. Inspection showed a remarkable change on the left side of the chest; it already looks smaller than the right, and there is flattening in the second, third, fourth, and fifth intercostal spaces; there is very marked pulmonary resonance to the fourth rib. Tympanitic in the fifth, sixth, and seventh interspaces.

With the exception of the fifth, when the temperature rose in the evening to 102°, the patient's condition was most satisfactory. He slept well, appetite good, temperature did not rise above 99°; there was free drainage through tube.

14*th*. Chest measured to-day: right side, fifteen and a half inches; left, fifteen and one-eighth. Discharge very light. From this time the patient improved very rapidly; temperature has not risen above 100°.

May 9. The discharge is now slight. A smaller drainage tube was introduced; the discharge gradually diminished, and he improved rapidly in strength and weight. Early in June the tube was removed.

June 10. The note is that the sinus has entirely healed. He left the hospital on the 15th, weighing 140 pounds, a gain of twenty-two pounds since April 14th.

The condition of his chest on discharge was as follows: There was marked flattening of the left side, particularly in the axillary and mammary regions. The circumference was: right, fifteen and three-quarters; left, fifteen and one-quarter inches. The percussion note was clear to the fifth rib and the spine of the scapula behind, below these points there was dulness. Loud breath sounds in the clavicular and mammary regions, feeble and distant in lower axillary, and at base.

I believe that this was an instance of pneumothorax from the outset, one of those interesting cases to which Dr. Samuel West[1] and Dr. de Havilland Hall[2] have called attention, in which the condition has followed strain in a person previously healthy. It is very improbable that on the eighth day of an acute pleurisy there would be a serous exudation of such extent as to reach the clavicle and encroach on the pleura of the other side. On the other hand, the percussion note, as is well known, may be dull in pneumothorax when the tension of the thoracic wall is very great, and I think that in this way the mistake arose. The mode of onset in a healthy man, the course of the disease, and the rapid and complete recovery favor the view that the strain had induced a pneumothorax which excited the pleurisy.

[1] Clin. Soc. Transactions, vol. xvii. [2] Ibid., xx.

The chief interest of the case lies, however, in the curious phenomenon which developed in the fifth week after the attack.

Instances of tumors of the thoracic wall, which pulsated synchronously with the heart, are mentioned by several of the older writers —Baillon (1640), Le Roy (1776), and Pelletan (1810)—but the first cases of pulsating empyema, recognized as such, were reported by the late Dr. R. L. Macdonnell,[1] Professor of Clinical Medicine in McGill University, Montreal, who, at the time, was clinical assistant to Drs. Graves and Stokes, at the Meath Hospital, Dublin.

In the first of these cases a large tumor appeared in the cardiac region, which, after pulsating for some time, became red, tense, and shining, and then burst, giving exit to a large quantity of pus.

In the second case two tumors appeared in the lower part of the left side, presenting fluctuation and pulsation. When opened purulent matter escaped in large quantities.

In the third case two large tumors appeared in the lower portion of the left side of the chest, presenting fluctuation and pulsation. They were opened and discharged a large quantity of pus. Death followed in all these cases.

Dr. Macdonnell remarked that this condition was new in the history of empyema.

Several careful studies of pulsating pleurisy have recently been made. One by Comby,[2] who collected 27 cases; and a second by Kepler,[3] who has collected 38 cases, only two of which are reported by American authors—Drs. Flint[4] and Dillingham,[5] from the wards of Dr. J. H. Ripley at St. Francis Hospital, New York.

I am able to add the reports of a few additional cases from this side of the Atlantic, but from inquiries which I have made from the hospital physicians of this country the condition appears to be extremely rare. Dr. George Ross, Professor of Clinical Medicine in McGill University, Montreal, has given an account of an extremely interesting case[6] which closely simulated aneurism:

A man, aged thirty-seven years, was admitted to the General Hospital suffering from pain in the side, cough and fever, which lasted about five

[1] Dublin Journ. Med. Science, March, 1844.
[2] Archives Générales, 1883.
[3] Deutsches Archiv für klin. Medicin, Bd. xl., 1887.
[4] Clinical Report on Chronic Pleuritis, p. 47; and On the Respiratory Organs, p. 581, 1856.
[5] New York Medical Record, 1884.
[6] Canada Med. and Surg. Journ., May, 1885.

days, and which followed a severe wetting. There was deficient expansion on the left side with dulness to the angle of the scapula and diminished fremitus. Within ten days the expansive movement of the left side became more impaired. The second and third intercostal spaces in front became prominent, presenting perceptible pulsation synchronous with systole of the heart. About five days after the onset of the illness he had a severe fit of coughing, in which he brought up, at least, a pint of pure pus, thick, creamy, and odorless. The cough continued for a few days, with expectoration of pus. The percussion note on the left side became clearer and the pulsating tumor entirely disappeared. The temperature fell to normal and the man's strength returned. Ten weeks from the onset the man left the hospital strong and well.

Dr. F. P. Henry, of Philadelphia,[1] reports a case from the Episcopal Hospital:

Woman, aged thirty years, admitted in the spring of 1880. On the left side of the thorax there were three strongly pulsating tumors—one about the size of half a large orange, in the left mammary region, directly over the central portion of the heart; a second, much smaller and acuminated—*i. e.*, with apex much smaller than the base—was situated on the left antero-inferior portion of the thorax; and a third, the largest of the three, on the left postero-inferior portion, its long diameter, about four inches, corresponding with that of the vertebral column. All these tumors possessed a strong expansile, systolic pulsation. The day after admission pus was withdrawn hypodermically from the smaller tumor. The tumor over the heart contained air which was very evident on manipulation. Aspiration was performed, and, some time after, a drainage tube was inserted by Dr. Ashhurst. The woman was removed by her friends, but was alive a year after the operation.

Dr. Janeway, of New York, writes that he has met with one case of empyema of the left side, in which the tumor was situated in the left second interspace, which pulsated when the patient stood erect, but when the patient was lying down air filled the sac.

These cases, with the thirty-eight collected from literature by Comby and Kepler, make a total of forty-two cases. The condition is almost invariably met with on the left side. In only three instances, those of Kepler, Heyfelder,[2] and Geigel,[2] was the empyema in the right side. Kepler thinks there may have been a doubt in Heyfelder's case, but the report seems perfectly clear. The tumor appeared between the second and third ribs on the right side, and pulsated distinctly. It may possibly have been a mediastinal abscess,

[1] Proceedings of the Phila. Co. Med. Society, vol. iii. p. 85. [2] Abstract by Kepler, l. c.

as it was close to the pleural margin. Only eight ounces of pus flowed out when punctured.

In the case of Geigel, a man æt. fifty-seven, had in the right mammary region a prominent projection which pulsated synchronously with the heart. The case terminated fatally. Between six and seven pounds of pus were found in the pleura.

Empyema existed in all cases, with the exception of one reported by Kepler, from Eichhorst's clinic, in which the fluid was serous. It occurred in a boy æt. fourteen, who, fourteen days before his admission, had been seized with a severe pain on the right side, and shortly afterward great tenderness at the seventh rib. There was dulness at the right base, which rapidly increased until it reached the angle of the scapula, and within a few days there were signs of effusion in the right thorax. On first examination the right side was enlarged, intercostal spaces prominent. There was active pulsation over the antero-lateral region of the right side of the chest reaching as high as the third rib, and synchronous with the movements of the heart. The apex beat of the heart was 1.5 cm. above the nipple in the left mammary line. On account of suffocative symptoms aspiration was performed and 800 c. cm. of pure serous fluid removed. Pulsation ceased after the withdrawal of the fluid. In fourteen days the fluid reaccumulated. An exploratory puncture showed it to be pus, and the operation for empyema was made. The seventh rib was resected and 300 c. cm. of pus removed.

In only two instances of Kepler's series was pyo-pneumothorax present. One reported by Féréol,[1] a man æt. twenty-two, had, in July, 1882, left-sided serous effusion, which was tapped, and he recovered. On October 27th there was again a large left side effusion with air. The beat was at the right nipple, and about the end of November pulsation of the whole left side was noticed, synchronous with the heart. It was most marked behind and in the axilla. The aspiration of $2\frac{1}{2}$ litres of pus abolished the pulsation. In Dillingham's case the man had pneumothorax.

To these cases must be added the one which I here report, and the cases of Henry and Janeway, in both of which there was evidently air in the pleura.

[1] Quoted by Kepler.

Two groups of cases may be recognized: 1, the intra-pleural pulsating pleurisy; 2, the pulsating empyema necessitatis, in which there is an external pulsating tumor. The latter condition, the most common, occurred in twenty-five of the forty-two cases, probably also in a larger proportion, as there are several reports with very scanty details. The external tumor is usually single, but in five cases there were two tumors, and in one, Dr. Henry's case, three. The perforation of the pleura usually occurs in the anterior aspect of the chest, from the second to the sixth rib, sometimes close to the sternum. In three cases the tumor appeared posteriorly—at the spine, at the angle of the scapula, and in the lumbar region. In the intra-pleural cases the pulsation is usually in the antero-lateral region of the affected side, and may be evident on palpation only, or, as in the case here reported, it may be visible even at a distance.

Pulsating pleurisy occasionally occurs in cases in which the fluid has existed for some time, but that it may occur in acute cases, even with a serous exudation, is illustrated by Kepler's patient. In Ross's case and in mine the condition was also acute.

Various explanations of the phenomenon have been offered. Dr. Broadbent[1] suggests that it occurs when adhesions exist between the layers of the pericardium and between the pericardium and the chest wall. But that this cannot hold good in all cases is shown by reports of post-mortems in which such adhesions were not present. Traube regarded destruction of the costal pleura, and marked paresis of the intercostal muscles as the conditions which rendered pleurisy possible. In the case which I have reported, there was persistent tenderness of the thoracic walls, suggestive, to say the least, of involvement in an unusual degree of the parietal structures, but there was no œdema or special protuberance of the spaces, and the condition came on too early to have been due to destructive changes in the pleura. It was probably due to extreme distention of the side. Bouveret, in his recent monograph on empyema,[2] holds that the pulsation is met with whenever the resistance of the thoracic wall is greatly reduced, as in the way Traube suggests, or when the resistance on the part of the diaphragm is heightened, as by the deposition of a thick layer of fibrin. The fact that the abstraction of a very small quantity of fluid

[1] Lancet, 1884.

[2] Traité de l'empyeme, par L. Bouveret, Paris, 1888.

will at once abolish the pulsation, indicates that a certain degree of pressure is a necessary condition. Comby thinks that the pulsation only occurs when the lung is compressed and adherent to the pericardium, so that the heart movements are communicated through it to the pleural fluid, and so to the chest wall. Féréol makes a somewhat similar suggestion, holding that in every instance the condition is one of pneumothorax, in which air forms an elastic cushion between the pericardium and the fluid through which the pulsations of the heart are directly transmitted to the chest wall.

The cases have been mistaken for aneurism, and the situation in which the pulsating empyema necessitatis usually develops renders the error very pardonable. The doubt can readily be solved with a fine hypodermic needle.

The prognosis in pulsating pleurisy is not very favorable. Of the thirty-eight cases in Kepler's series, seventeen died. But we must remember that most of these cases occurred before the days of safe and frequent operations upon the chest wall.

Complete evacuation of the fluid with free and permanent drainage meets the indications for treatment.

ANTISEPTIC MEDICATION.

By FREDERICK P. HENRY, M.D.,
PHYSICIAN TO THE EPISCOPAL AND PHILADELPHIA HOSPITALS.

My purpose in bringing the subject of antiseptic medication before this Association is not so much to make an elaborate report on the existing knowledge concerning it, the principles on which it is based, the results it has accomplished, and my own experience of its application, as to elicit a general expression of opinion upon what appears to me the foremost therapeutic question of the day.

The great principle of antisepsis is of general application in the treatment of disease. It is not limited to the domain of surgery, although to surgeons is due and gratefully accorded the merit of having shown the dangers due to absorption of products of fermentation and putrefaction and the means of avoiding them. The revolution thus brought about has been witnessed by every member of this Association. It is an accomplished fact and, at the present time, the applications of antisepsis in this field are so perfect, so precise, that surgery, in some of its departments, may almost be ranked among the exact sciences.

Antisepsis, in surgery, consists in preventing the absorption of toxic substances from a pathological surface. This is true also to a certain extent in medicine, although here, in many instances, the surface from which absorption takes place is physiological. The difference between antiseptic medicine and antiseptic surgery is one of degree rather than of kind. In the former the antiseptic agent is directly applied to the absorbent surface; in the latter it can, in most instances, only exert its action after having run the gauntlet of the greater portion of the intestine, or even after it has been absorbed and excreted. The problem in the latter case is much the more difficult of the two, but it is one which, as I propose to show, is in the course of successful solution.

Lavage.

A well-defined line can be drawn between gastric and intestinal antisepsis for the reason that in the former we have one remedial measure *par excellence* to which all others are subordinate. I refer to lavage, and it is an interesting fact that Dujardin-Beaumetz, who opposed this method of treatment when first introduced by Kussmaul in 1867, is now one of its most ardent advocates. He believes it to be indicated whenever the stomach is dilated, no matter what may be the cause of the dilatation. The operation, which may be compared to catheterism for the relief of a distended bladder, is not to be performed without certain precautions. In the first place, it should not be performed too frequently. Once in twenty-four hours is often enough. Secondly, the water employed should be at, or near, the temperature of the body. Thirdly, in order not to interfere with the digestion of which the stomach may be capable, it should be performed during a period of abstinence from food, *e. g.*, early in the morning. These precautions are by no means founded on theory alone. If lavage is repeated too often, it not only fatigues the patient, but may lead to grave accidents. For example, in a case reported by Dujardin-Beaumetz to the *Soc. Méd. des Hôpitaux*, October 26, 1883, the patient experienced great relief from the treatment and having learned to wash out his stomach for himself, acquired the habit of doing so several times a day. This abuse of the method gave rise to muscular contractions which, beginning in the upper extremity, soon became general, and the man died with all the symptoms of tetanus. Lavage has been most often employed in the treatment of dilatation from pyloric stenosis, and certainly with most satisfactory results. The painful distention of the stomach, the nausea, regurgitations, and vomitings are, in most cases, allayed by this simple means.

There is another form of gastric disorder often overlooked and only recently clearly differentiated, in which lavage is of most signal service, viz., that in which the gastric juice, instead of being secreted only when there is food in the stomach, is poured out when the viscus is entirely empty. This condition of hypersecretion must not be confounded with hyperacidity. The latter consists in a too liberal response to a physiological summons, while the former is gratuitous. First described by Reichmann in 1882, cases have since been reported

by Sahli, Schütz, von den Velden, and especially by Riegel, who, in the course of eighteen months, observed twenty-nine cases. According to the latter, lavage should be practised in cases of hypersecretion just before the principal meal and in no other affection is it attended with greater benefit.

For washing out the stomach simple water is sufficient, but when the viscus is emptied it may be finally cleansed with an antiseptic solution, such as salicylic acid three-fifths per mille, or borax two-fourths per cent. These harmless substances are quite efficacious, although benzoic acid, naphthalin, potassium permanganate, and resorcin are recommended. The latter substance is unsafe, except in very weak solution. Escherich[1] reports a case of poisoning from lavage with a one per cent. solution of resorcin. The patient was a woman, æt. thirty-five, who, after lavage, became restless and cyanotic and soon lost consciousness. Clonic and tonic spasms then set in, the latter affecting particularly the masseters and the respiratory muscles. There were trismus and prolonged spasm of the glottis with stoppage of respiration, during which the face was extremely livid and cold drops of sweat stood upon the forehead. After life had been maintained for about two hours by continued artificial respiration, natural breathing and consciousness returned, but the action of the heart now became weaker and it was only by the most energetic measures that life was saved. For hours the patient lay in a stupid, drunken-like ("rauschartig") condition.

Intestinal Antisepsis.

The principal field of antiseptic medicine is the intestine, and this fact, which is one of increasing importance, has been recognized ever since Selmi, in 1872, discovered in the products of putrefaction, and even in fresh vegetable tissues, certain substances closely resembling the vegetable alkaloids to which he gave the name of ptomaines.

It is, however, justly claimed, that Semmola, who twenty years ago administered the alkaline and earthy sulphites with the object of destroying certain supposititious substances of a toxic and pyrogenic nature, was the first to institute a treatment based upon the idea of intestinal antisepsis.

[1] Centralblatt für Bakteriologie und Parasitenkunde, Bd. ii., No. 21.

At the present day it is very generally held that many forms of diarrhœa are due to irritation by substances which have escaped the powerful antiseptic action of the bile and gastric juice, while it is believed by some that many of the grave nervous symptoms of the continued fevers, the so-called typhoid symptoms, are due to the absorption of similar products.

The therapeutic problem is to administer such substances as will supplement the defective action of nature's antiseptic secretions, a problem which, as I have already stated, is in the course of successful solution. It is not my intention in this paper to enumerate the many substances that have been used to fulfil this indication, but merely to give some details concerning the few of which I have had some experience.

NAPHTHALIN.—This substance was first employed by Rossbach in 1884 as an intestinal antiseptic. He obtained excellent results from it in the treatment of chronic intestinal catarrh, either with or without ulceration, and in typhoid fever, in which disease, when early administered, he believes it exerts an abortive effect. Thirty-five cases of typhoid in Rossbach's medical service at Jena were treated with naphthalin by Götze, all of which took over seventy grammes in the course of their treatment; many of them over a hundred and fifty grammes. The favorable effect of the drug was shown by the number of the stools, which averaged from one to three daily. Severe diarrhœa, requiring the addition of opium to the treatment, was observed in but three cases. The pain in the ileo-cæcal region rapidly diminished, disappearing, as a rule, on the second day. On account of the dark color of the urine produced by the drug, this fluid was frequently examined and the albuminuria in this series of cases was no more frequent or severe than in cases of typhoid in general. In one case in which death was caused by croupous pneumonia there were acute nephritis and typhoid ulceration of the renal pelvis, the former being due to the latter.

Of the 35 cases, 17 (50 per cent.) were so materially shortened in their course that the term "abortive" is fairly applied to them, while the course of the fever was favorably influenced in the others. Of the 35 cases, 3 died, but not, it is claimed, from typhoid. In the first, a vulvo-vaginitis extended to the large intestine and bladder, which were found in a state of diphtheritic inflammation. Of the

other two, the first died of croupous pneumonia of the right upper lobe; the second of double broncho-pneumonia and tracheal diphtheria. The claim that these deaths were not due to typhoid fever is, in my opinion, preposterous, and, therefore, the mortality, instead of being nil, as Götze would have it, is about 9 per cent. This is small, but the cases are too few to warrant any deductions under this head. The favorable course pursued by the great majority of Götze's cases would lead one to expect still better results from a more extended use of the drug.

There is a large concourse of testimony to the good effect of naphthalin in chronic intestinal catarrh, the results obtained by Rossbach, Pauli, Pribram, Kraemer, Widowitz, and Wilcox having been invariably good. An extensive experience with this drug during a recent six months' term at the Philadelphia Hospital, enables me to corroborate their statements.

In the *St. Louis Med. and Surg. Journal* for March, 1887, Dr. R. W. Wilcox, of New York, gives his experience with this drug in the treatment of thirty-two cases of diarrhœa, most of them chronic. "All of the cases were relieved in periods of from one week to two months; generally about ten days were sufficient for a cure." The cases of Wilcox, like my own, were nearly all adults.

Dr. Josef Widowitz[1] has employed naphthalin in thirty-three cases of intestinal catarrh in children, and concludes that in the so-called dyspepsia of children with clay-like stools and vomiting of curds, naphthalin is without effect, while in all other forms of intestinal catarrh in children it deserves the preference over all other remedies. This apparent exception to the beneficial action of the drug is, in reality, none, for the condition referred to is gastric or gastro-duodenal catarrh. Widowitz administers naphthalin to small children in the following formula:

℞.—Naphthalin. pur.	0.30–1.
Mucilag. gummi Arab.,	
Aq. chamom.	āā 40
Olei menthæ pip.	gtt j—M.

After shaking well give a teaspoonful every two hours. The amount given during the day should be from 0.3 to 1 gramme.

[1] Jahrbuch für Kinderheilkunde. Bd. xxvi. Hefte 3 und 4, 1887.

If one may judge from the practice of those who have employed this drug, their opinions with reference to its dose are very different. For example, the dose employed by Kraemer,[1] whose success with naphthalin has been brilliant, did not exceed one gramme daily. With this amount he speedily cured ten cases of intestinal catarrh which had lasted a year in spite of other treatment. He also succeeded in completely expelling the oxyuris vermicularis in cases which had resisted all the usual vermicides employed both per os and per clysma.

On the other hand, Wilcox believes that the most frequent cause of failure with this drug is due to the use of too small doses, and considers the administration of less than sixty grains daily as a "needless waste of very good medicine." Accurate dosage is certainly not essential to the administration of this drug. It is insoluble, not only in water, but in acids and alkalies, so that it reaches the intestines unaltered and, in fact, appears, for the most part, unchanged in the feces. A minimum part of it is absorbed and appears in the urine partly as such and partly as alpha-naphthol. Although the amount that passes out with the urine is small, its antiseptic effect is so great that, according to Rossbach, the urine of patients who have been taking naphthalin may be kept for weeks, and even months, in a warm place without losing its acid reaction and without decomposition.[2] In accordance with these facts, the internal employment of naphthalin in light chronic cystitis has been attended with excellent results. In one or two days the cocci disappear, the pus sediment diminishes, and there is marked improvement in the local and general conditions. In severe, chronic, incurable cases, temporary improvement is the most that can be expected, as in a case of tuberculosis of lungs, bladder, and kidneys reported by Rossbach.[3]

With reference to certain unpleasant effects that have attended the administration of naphthalin, Rossbach states that, with the exception of slight burning in the urinary passages lasting only a few days, and fugitive pains in the neighborhood of the kidneys, serious symptoms due to the drug itself were observed in only 2 out of 150 cases. Severe strangury was observed in these 2 cases, both of which were tuberculous. One was in the last stage of pulmonary and intestinal

[1] Berlin. klin. Wochenschrift, Jan. 18, 1886.

[2] Einfluss des innerlichen Naphthalingebrauches auf die Harnfäulniss. Rossbach, Berlin. klin. Wochenschrift, Nov. 17, 1884.

[3] Loc. cit.

tuberculosis; the other, in whom an inflammation of the prepuce set in on the third day of administration of the remedy, bore it well after it was suspended and resumed in smaller doses. Evil effects from this drug are almost invariably due to the fact that it is impure. A case in point is that of a butcher's powerful apprentice, who, after the third gramme of naphthalin, suffered from severe strangury. An examination of the drug showed that it was impure, being gray instead of white. On the day following, the same patient took seven grammes of a pure preparation without a bad symptom.

While large doses of naphthalin are being administered, the urine is of a dark, even black, color, but this fact is of no evil significance. When administered with ordinary care, the worst that can happen is occasional burning pain in the urinary passages, and to refuse, on that account, to employ in chronic diarrhœa a remedy which has cured many of the most obstinate cases, would be, to quote Rossbach, about as sensible as to banish iodine and potassium iodide from therapeutics because now and then they give rise to acute coryza and palpitation of the heart.[1]

The mode in which naphthalin and other drugs intended to act directly on the intestine are prescribed, is by no means unimportant. Probably the best method of administration is that recommended by Unna. The drug is made into a mass with some fat, such as cocoa butter, which melts at the temperature of the body, and each individual pill is surrounded with a layer of the same substance. They are then coated with a layer of keratin (Hornsubstanz), which is insoluble in the acids of the stomach. The keratin is applied to the pills in solution in ammonia, but before this it is digested in an artificial gastric juice to free it from substances which, but for this process, would be dissolved in the stomach. The keratin coating of these pills is dissolved by the alkaline fluids of the small intestine and their fat is saponified so that the remedy is set free to exert its local action. Unna has employed in this manner tannin in intestinal catarrh and ext. filicis maris in cases of tapeworm, with good results, and recommends that astringents and styptics be thus administered, especially in intestinal hemorrhage.[2] Dr. F. Kræmer, of Munich, has employed these pills and thinks that by this mode of administration

[1] Berlin. klin. Wochenschrift, April 6, 1885.

[2] For description of these pills see article by Pribram, Wiener med. Presse, February 1, 1885.

many of the unpleasant effects attendant upon the use of naphthalin are avoided.

Probably the most common mode of administering naphthalin is in gelatine capsules. It is important, therefore, to remember that gelatine capsules should not be given to a patient who is taking alcohol, as they are rendered insoluble by that substance. Tannic acid has the same effect. This subject is referred to in an editorial in *The Southern Practitioner* for September, 1886, in the course of which the writer mentions the fact of a physician having prescribed tannic acid in gelatine capsules. Gelatine capsules are not only insoluble in a stomach which contains alcohol, but also in one that is suffering from the effects of alcohol. "When the stomach is inflamed and irritable, following excess in the use of spirits, medicines should not be given in capsules (gelatine)." "Neither chloral nor morphia should ever be given in this form to inebriates; it is useless."[1]

THYMOL.—In *The Medical News* of September 3, 1887, I published my experience with thymol in the treatment of twelve cases of typhoid fever. My attention was first called to this drug by Martini's elaborate article in the *Annali Universali di Medicina e Chirurgia*,[2] February, 1887, and I was induced to publish my observations with it, not only because they were corroborative of Martini's statements, but also because they were confirmed by my colleague, who succeeded me at the Episcopal Hospital and who continued to employ the drug. Since that time I have used thymol in eight additional cases of typhoid fever and in a large number of cases of diarrhœa, acute and chronic. With reference to the former I have only to quote from my article in *The Medical News:* "The favorable effect of the drug was evinced by a steady descent of the temperature, by a gradual diminution in the daily number of stools, by the absence of mental excitement, and, most conspicuously, by the clean, moist tongue presented in every instance." In the series of twenty cases there has been one death which was caused by one of the rarest events in the course of typhoid fever—rupture of the spleen.

That thymol when taken *per os* exerts an antiseptic action upon the intestinal contents, is proved beyond doubt by the fact that during its administration phenol disappears from the urine. "This sub-

[1] Medical Record, January 15, 1887.

[2] Dell'efficacia del timolo nella disinfezione intestinale.

stance, one of the most constant products of intestinal putrefaction, is eliminated, for the most part, by the urine, where it is found even in health, and experimental therapeutists regards its amount in that fluid as a test of the effect of an intestinal antiseptic."[1]

The symptoms known as "typhoid" are common to a number of diseases and their distinct amelioration after the administration of an intestinal antiseptic is strong evidence in favor of the view that they are largely due to poisoning from absorption of products of intestinal putrefaction. On account of its great insolubility, a property upon which its value as an intestinal antiseptic largely depends, thymol must be prescribed in pill or capsule. One of the best excipients for thymol is medicinal soap. As to the dose, my practice has been to give from 30 to 40 grains in twenty-four hours—two 2½ grain pills every three or four hours, although much larger quantities may be given with perfect safety and, perhaps, with still better results. I have given thymol to all the cases of diarrhœa, acute and chronic, under my care during a six months' service at the Philadelphia Hospital, and, excepting a few cases of intestinal tuberculosis, with invariably good results. I regret that I cannot obtain the number of these cases in time for publication. To those familiar with the hospital in question, it is scarcely necessary to say that my opportunities for testing the drug were great.

MERCURY.—As I have no present intention of writing a treatise on antiseptic medicine, I will neither refer to the various modes of administering mercury nor discuss the question how far its beneficial effect, in general, may be of an antiseptic nature, but will simply give my experience of its employment in the treatment of erysipelas.

The evidence that erysipelas is a local disease due to a microörganism is overwhelming, but, if I may judge from a recent discussion in a Philadelphia medical society, it has not yet come to the knowledge of some of the most prominent members of our profession. I will, therefore, briefly recapitulate the result of an investigation of the subject.

It has been thoroughly demonstrated by Fehleisen and others that erysipelas is caused by the presence of a living organism, micrococcus, in the lymphatic vessels of the skin. This organism, which is

[1] Editorial in The Medical News on Intestinal Antisepsis, June 11, 1887.

usually observed in the form of chains or strings of beads, and is, therefore, known as a streptococcus, has been isolated by successive cultures and inoculated into rabbits with the result of giving them an attack of erysipelas. The animals inoculated were nine in number, and of the nine, eight had a well-marked attack of the disease, starting from the tip of the ear in which the inoculation was made. The period of incubation—*i. e.*, the time between the inoculation and the first signs of inflammation, varied between thirty-six and forty-eight hours. Denucé, a French observer, has repeated these experiments upon six animals, five rabbits and one dog, with positive results in every case. Such evidence would be accepted by most individuals as sufficient to prove the dependence of erysipelas upon a microörganism; but Fehleisen has gone a step further, and inoculated seven human beings. He did this on the ground that it was not only for the benefit of science, but for that of the individuals themselves, although it does not appear that they were consulted on this point. It has long been observed that chronic eczematous affections, glandular swellings, lupus, and multiple sarcomatous tumors have disappeared after an attack of erysipelas, and it was upon individuals afflicted with such disease that the experiments were made. All but one of these experiments were successful from the standpoint of experimental pathology, but the therapeutic results hardly warrant the addition of the streptococococcus of Fehleisen to the Pharmacopœia. In fact, a woman forty years of age, with scirrhus of the breast, who was inoculated with erysipelas by Janicke and Neisser,[1] died four days after the operation. It was a satisfaction to these experimenters to observe at the autopsy that the micrococci artificially introduced had penetrated the cancer cells and destroyed them in large numbers. The remedy, however, if such it can be called, was undoubtedly more malignant than the cancer itself.

The above facts are quite sufficient to prove that erysipelas is a local parasitic disease, and to justify a local germicidal method of treatment. A treatment based on these views of the nature of erysipelas was first recommended by Hueter. It consisted in the subcutaneous injection of a two per cent. solution of carbolic acid, manifestly with the object of destroying the parasite. Its success was not

[1] Centralblatt für Chirurgie, 1884, p. 401.

striking, and an attempt to improve it was, therefore, made by Dr. Kühnast, of Freiburg, who concluded from a study of three cases that multiple scarification and the application, with rubbing, of a five per cent. solution of carbolic acid, were the best treatment yet devised. This may be true; but, if so, there are numerous exceptions to the rule. In the first place, this method cannot be applied to that part of the body—the face—which is the most common site of erysipelas, on account of the danger of subsequent scarring. Secondly, it is so painful that the operation must be performed under ether. Finally, in the case of children and weakly individuals its upholder recommends that boric or salicylic acid be substituted for carbolic. Surely the objections to this treatment are so great that it can never come into general use.

Thinking that the want of success attendant upon the subcutaneous injection of carbolic acid might be due to the inherent inefficacy of that drug, I have lately been treating erysipelas with hypodermatic injections of the bicyanide of mercury. My reason for employing this salt in preference to corrosive sublimate is that the former is compatible with cocaine while the latter is not. A hypodermatic injection of corrosive sublimate is so painful that few will consent to its repetition, while the bicyanide, as I have employed it, is comparatively painless. The following formula is the one I am accustomed to use:

℞.—Hydrarg. bicyanid. . . . . grs. ij.
Cocain. hydrochlorat. . . . . grs. iv.
Aquæ destillat. . . . . ʒss.—M.
Fifteen minims to be injected beneath the skin.

I have made about two hundred injections of this mixture in cases of syphilis and erysipelas, always using the dose above mentioned, and have never seen any untoward constitutional symptoms from it, beyond a momentary faintness which I attributed to the cocaine. Several of the patients were delicate women.

I have treated ten cases of erysipelas capitis[1] with injections of the bicyanide, and without entering into details I may state that when the disease is beginning, for example, at the side of the nose and extending toward one or both cheeks, this treatment tends to abort it.

[1] My thanks are due to Drs. Atlee and Dercum for their assistance in carrying out this treatment.

It is of great importance that the injection be made, not into the inflamed patch, but a little beyond its border.

Erysipelas, from a pathological standpoint, consists of two stages. During the first there is an accumulation of micrococci in skin which is apparently healthy. During the second there is a displacement of the micrococci by out-wandered white cells which dispose of the former by incorporating them into their substance so that under the microscope they present a granular aspect. In order, therefore, to abort an attack of erysipelas the injection must be made in the apparently healthy skin at least one-half inch beyond the advancing border of inflammation. I have seen at least three cases of erysipelas which were aborted by the treatment above outlined. These, all hospital cases, were seen at an early period of the disease, while, in the others, it was widespread at the time of admission. I do not consider this treatment effectual after the disease has lasted more than forty-eight hours. It is a singular fact that in the majority of my cases, the injections of the bicyanide gave rise to abscesses. I consider the fact singular because the substance injected is a powerful antiseptic, and because I have given about one hundred and fifty of the same injections to two women with syphilis, without the slightest suppuration ensuing. In using the injection strict antiseptic precautions were employed: among others the needle of the hypodermatic syringe was heated in a spirit lamp.

The following abstract will serve as a sample of those cases treated at a comparatively early stage:

John H. W., æt. fifty-one years, admitted April 19, 1888, with erysipelas in both cheeks, but more extended on left side. The attack began four days ago at the site of a recent cicatrix resulting from an injury received about four weeks previously. Febrile reaction was slight, the temperature at evening being 100°. The urine was free from albumin. Fifteen minims of the bicyanide solution were injected into each cheek beyond the border of the inflammation.

20*th*. The cheeks are not so much inflamed and the inflammation has not extended. Two injections were given, both on the left side; one in the temporal region, the other in the neck.

21*st*. The inflammation is gradually subsiding in the cheeks and *has not extended its area*. The temperature fell to normal on the 22d, and did not subsequently rise above 99°. The man was discharged from hospital on the 30th of April.

In another case, seen when the inflammation had not lasted more than twenty-four hours and involved only a small area about the ala nasi, one injection effected a cure.

Certain facts seem to show that the micrococcus of erysipelas develops most rapidly when the part affected is exposed to the air; or, rather, this is an inference from the effects of a treatment which excludes the air from it. For example, Mr. Barwell, of Charing Cross Hospital, has treated erysipelas by the application of white lead paint, with the result of speedily relieving the pain and reducing the fever; the patient rapidly recovering. W. Otto, of Germany, following the suggestion of Barwell, has excluded the air with a mixture of two parts wax, twenty parts of "Siccativ," and one hundred parts of linseed oil varnish ("leinölfirniss"). His cases, five in number, all did well, the disease in no case lasting longer than ten days.

One of the latest remedies proposed for erysipelas is ichthyol,[1] which is a yellowish-brown oily substance obtained by distilling a bituminous matter found in the Tyrol, and composed of the fossilized remains of fishes and marine animals. It is used in the form of the sodium or ammonium sulph-ichthyolate. Von Nussbaum claims that this substance will abort erysipelas, but it did not do so in the only case in which I gave it a thorough trial. The course pursued by this case was, however, remarkably favorable. Von Nussbaum supposes that ichthyol alters the tissues in such a manner as to make them an unfit medium for the growth of the parasite. It is, however, quite as likely that both it and the white paint, as well as the liniment used by Otto, act by excluding the air. Whether this be the case or not, such measures are probably antiseptic in their action, although inferior in this respect to a method such as that I have described and employed, which brings the germicide in direct contact with the germ.

The lack of time and space forbids a further consideration of this interesting subject. Otherwise, under the head of intestinal antisepsis, I would have referred to some interesting facts concerning the action of cotoin, salol, and bismuth salicylate.

In conclusion, I will merely refer to certain observations which not only show that microörganisms may subserve some useful purpose in the economy, but convey the hint that, by taking advantage of their

[1] See paper by Dr. Edward Martin, in Journal American Medical Association, 1888.

opposing tendencies, we may yet compel them—the germs of fermentation and putrefaction—to play the *rôle* of antiseptics.

The first set of observations to which I refer are those of Rietsch.[1] In this investigation cultures of the microörganisms of cholera Asiatica, of typhus, and of the staphylococcus aureus in 2.5 per cent. peptone solution, were precipitated with alcohol. The precipitate was separated by filtration, washed repeatedly and dried, and the powder thus obtained was tested with reference to its power of digesting albumin. The result was that neither the tubercle nor the typhus bacilli were capable of digesting fibrin, either in alkaline or neutral solution. On the other hand, the cholera bacillus and the staphylococcus aureus, in neutral solution, digested fibrin and the resulting fluid gave the reaction of peptone. In weak alkaline solution the fibrin was also attacked, and in neutralizing with HCl a precipitate took place which gave the reactions of syntonin and globulin. The digestive capacity of these bacteria, thus demonstrated, had already been suspected on account of the liquefaction of the gelatine in which they are sometimes cultivated.

The second set of observations are those of Hirschler,[2] who found that in mixtures of albuminoids and carbohydrates, the bacteria which first develop are invariably those that decompose the latter. For example, in milk, notwithstanding the presence of numerous casein-decomposing bacteria, the acid fermentation first sets in. In such cases the withdrawal of carbohydrates and the substitution of a purely albuminoid diet is indicated. On the other hand, in cases of putrefaction of albuminoids, with offensive stools, all proteids are to be temporarily withdrawn and carbohydrates substituted. The acid fermentation of the latter is the result of bacteria which are inimical to those causing the horribly offensive stools. These most suggestive facts lead directly to the belief that the time is not far distant when what we now somewhat vaguely speak of as a well-regulated diet will be one selected not exclusively with reference to the nice adjustment of its proteids and carbohydrates, but also with a view to the mutual relations of the minute organisms which subsequently develop within it.

[1] Contribution à l'étude des ferments digestifs sécrétés par les bactéries. Journal de Pharmacie et de Chimie, 1 Juillet, 1887.

[2] Quoted by Escherich—Beiträge zur ansiseptischen Behandlungsmethode der Magen-darmkrankheiten des Säuglingsalters. Jahrbuch für Kinderheilkunde, Bd. xxvii. Hefte 1 und 2, 1887.

PHTHISIS PULMONALIS.
THE BASIS FOR ITS THERAPEUTICS.

By S. EDWIN SOLLY, M.D.,
COLORADO SPRINGS, COL.

Before discussing the general therapeutics of phthisis, it appears necessary to give a clear definition of the disease and its tendencies. As I find that the limits of one paper will not allow more than the consideration of the disease, I must leave the question of the remedies for another occasion. The following, therefore, is an endeavor to trace out the chief causes, both within and without the body, which admit of the lodgement of the bacillus in living tissue and which favor or retard its growth when there.

The discovery of Koch has established the unity of all forms of pulmonary tuberculosis as regards its exciting cause. To explain the reasons for the many-sided aspects of the disease, both clinically and pathologically, as well as the variation in the individual characteristics of its victims, so as to formulate a working scheme whereby treatment can be more rationally applied, would appear to be the task now lying ready for clinicians. The following must be accepted, not as finite in definition or conclusion, but as suggesting the main lines on which work is needed.

The conclusions forced upon us by recent pathological research are briefly as follows:

The varied forms of chronic wasting of lung tissue, with their consequent general tabes, which we group under the title of Phthisis Pulmonalis, are, excepting non-tubercular fibrosis, always associated with tuberculosis.

The bacillus tuberculosis is the invariable exciting cause of tubercle. Experiment points to the conclusion that a lesion of the mucous or cutaneous surface is usually necessary to effect the lodgement of the bacillus in the body, though there is evidence to show that a lesion is

not an absolute essential to inoculation, as tuberculosis is possibly conveyed through the process of both impregnation and digestion.

Certain unknown conditions of the tissues or disturbances of the functions of the body are necessarily present to enable the bacillus to establish the disease.

Admitting that the bacillus is the seed without which the disease cannot exist, we know that this seed will only grow when the soil is in a suitable condition to propagate it. What the actual essentials of this condition are, we do not know. But there are certain general antecedent circumstances which appear to exercise powerful determining influences in procuring this condition of the soil or tissue.

In the first place, a certain physique, generally, but not always, inherited, which we term the strumous or the tuberculous diathesis, is the most frequent, though it is not by any means an essential factor in the problem, as we often see men of apparently perfect physique and of a healthy, long-lived ancestry stricken with the disease.

A second common antecedent circumstance is a catarrh or inflammation of some portion of the respiratory tract, or continued breathing of some irritating substance, as in the business of knife-grinding, etc.

A third is a long-continued dyspepsia, with its consequent malnutrition.

A fourth, the continued breathing of a vitiated atmosphere.

A fifth, depression of the nervous system, notably through the mind, as by grief and business worry.

A sixth, through a general depression of the vital forces, as by fevers, injuries, poisons, etc.

In some cases of phthisis several of these antecedent circumstances have been present, but in some few the most careful inquiry fails to establish the existence of any of them.

The evidence of contagion being the sole cause, without the presence of one or more of the circumstances just detailed, is very slight, as shown by the reports from consumptive hospitals and sanitaria. In the cases of supposed contagion from near relatives and friends, the question is always complicated by the accompanying conditions of mental and nervous depression, vitiated air, or general depression of the vital forces from insufficient food and sleep, or over-work.

There are necessary to establish tuberculosis in the body, three factors: The seed—that is, the bacillus; the sowing—that is, the

inoculation; and the soil—that is, the tissue in a certain condition of receptivity. From these comes the crop—that is, the disease.

First, as regards the seed, it is fair to assume that we are almost constantly exposed to it, and the fact of the disease occasionally arising in climates within a supposed zone immunity, shows that it may probably be encountered in any part of the earth's surface. In experiments made, however, the number of the bacilli appear to influence the chances of inoculation, and therefore the relative purity of the air is of importance.

As regards the vitality of the bacillus, it is shown that moisture increases, and dryness decreases it; that a certain degree of temperature and a certain strength of germicide are needed to cause its death; and neither can be applied to effect this while the bacillus is in the living body without also destroying the tissue it inhabits.

In respect to the sowing, or inoculation. Direct inoculations with bacillary material, both those intentional upon animals and those accidental upon man, show always more or less local development of tuberculosis around the point of inoculation, and in certain cases secondary development therefrom, generally in the pulmonary and intestinal systems, and in these, if not interfered with, a certain number develop the symptoms of phthisis and die in consequence. The evidence shows the special affinity of the bacillus wherever introduced toward the pulmonary tissues, and also that direct absorption by the membranes of the respiratory tract, of bacilli from inspired air, is the commonest origin of phthisis. The fact that many animals forced to breathe bacillary air escape infection points to the conclusion that a lesion of the membrane is generally, if not always, needed.

Evidence tends to show that inoculation may occur from the eating of bacillary-infected meat and milk, but the fact that tuberculosis is known to be so common a disease among cattle, and the relatively small proportion of the human beings who eat beef and drink milk, who contract tuberculosis, would suggest that here also that in most, if not all, a local lesion is necessary to procure inoculation.

Let us briefly consider the conditions necessary to allow of secondary development from the primary lesion. It has been observed both in man and animals that tuberculosis is sometimes limited to the immediate neighborhood of the point of inoculation, there being always exhibited a transient local inflammation. Dissection of the part in

such cases indicates that the circulation reacting to the irritation of the foreign substance introduced, has thrown out walls of defensive lymph around the intruder and sealed the channels for absorption into the lymphatic or sanguineous system.

The influence of environment upon this conservative reaction of the body invaded by the bacillus is shown in various ways, most directly by Trudeau's experiments. Though all insanitary environments evidently tend to increase the chances of secondary infection, yet as the pulmonary tissues are the natural selective point of the bacillus, so we find those affecting respiration are most potent.

This is brought about by both direct and indirect contamination of the air used by the infected being. Direct, by impregnation of the air by foreign gases of an injurious character, and indirect, by not changing the air sufficiently often to prevent reinspiration of air previously expired.

There is a further method of indirect contamination of the air by the individual failing to take in sufficient air at each inspiration to procure a complete exchange of oxygen for carbonic acid, etc. Thus, although the air brought to the individual may be free from personal and foreign contaminations, yet unless his chest expansion is sufficient to bring about an equal exchange of oxygen for carbonic acid, etc., he will be the subject of autogenetic air contamination.

Chest expansion being increased or diminished by exercise; we see, although admitting the microbic origin of phthisis, how the purity of the atmospheric air, the frequent change of the air of the neighborhood by wind, or the dwelling by ventilation, and the frequent and sufficient depth of respiration through exercise, determine its inception or its progress. Therefore, the three external safeguards against the lodgement of the bacillus in the respiratory tract, are pure air, good ventilation, and a full and regular performance of the act of respiration.

To pass from the method of inoculation and the conditions influencing it, to the soil or tissue. As has been said, research points to the conclusion that in most, if not in all cases of absorption of bacillary matter, there is probably some abrasion of the surface, and this, no doubt, explains why the history of phthisis so often discloses the fact of a catarrh, or inflammation, in the course of the respiratory tract,

or a catarrh, inflammation, or dyspepsia of the alimentary canal, as a precedent condition to an attack of phthisis.

Granted a lodgement of the bacillus within the body, what prevents and what favors its growth *in situ* and its transference to other parts of the system? In the first place, we know that normal, healthy tissue resents the intrusion of any foreign substance, and either extrudes it by ulceration or builds a Chinese wall around it, of which fibrous tissue is the chief component. This is shown in certain experiments by inoculation, and may be inferred from the fact of inert tuberculous masses being found in the lungs of persons whose history does not reveal any record of phthisis. Therefore, it follows that not only are all means which promote a healthy condition of the tissues a preventative against phthisis, but also a powerful means of arresting its progress locally and stopping its transference to other parts of the system after its establishment.

The part that heredity plays in promoting the disease is quite uncertain, but it is fair to assume that direct transmission is rare, but that the inheritance of certain peculiarities or defects in the organism either general or local, leave the inheritor less able to defend himself against the progress of an established tuberculosis. Possibly there are peculiarities of primary cell-form and cell-life, and probably there are special peculiarities of the lymphatic system, both of structure and function, the phenomena of struma particularly pointing to this. Knowing, as we do, the common connection of tuberculosis and chronic local congestions, and also of the tendency to, what the Germans call, increased venosity, it is not surprising if the inheritance of a lymphatic temperament should give a special predisposition to phthisis.

The grouping of individual peculiarities under the name of temperament is founded upon the reaction of the nervous or circulatory systems of the individual, to a stimulus, being exaggerated or defective. This applies to both mental and bodily phenomena. This reaction in the absolutely normal man is equal in force and continuance to the cause. In the abnormal man, the reaction is either in excessive or deficient proportion to the cause. As most of us are more or less abnormal, at least in having marked tendencies in one or another direction, we may most of us be said to be possessed of one of the temperaments to be described.

1. The nervous temperament, in which the reaction to a stimulus applied to the nervous system is excessive. The American an example.

2. The phlegmatic temperament, in which the reaction to a stimulus applied to the nervous system is deficient. The Dutchman an example.

3. The sanguineous temperament, in which the reaction to a stimulus applied to the circulatory system is excessive. The plethoric person—John Bull an example.

4. The lymphatic temperament, in which the reaction to a stimulus applied to the circulatory system is deficient. The anæmic person—half-breeds often an example.

It will be found that all varieties of temperaments fall within these definitions, though doubtless subdivisions could easily be made, and the special influence of particular organs considered.

As each individual has both a nervous and a circulatory system, the real temperaments as they exist, are mixed, and may be named as follows:

a. The nervo-sanguineous, which, representing both systems acting with force and energy, is the normal temperament in exaggeration.

b. The phlegmo-sanguineous, in which the nervous reaction is deficient, while the circulatory is forcible.

c. The nervo-lymphatic, in which the nervous reaction is in excess, the circulatory feeble.

d. The phlegmo-lymphatic, in which the reaction, both nervous and circulatory, is feeble and slow.

Tuberculosis being primarily a microbic disease, of course explains why a consumptive may have any one of these four mixed temperaments. But when we consider how a tendency to chronic congestion and increased venosity of the pulmonary tissue would favor the growth of the bacilli when once established, and how the feebleness of the circulation would make the defence by firm plastic exudation or rapid removal very slow and imperfect, we can understand why the bulk of consumptives are of the lymphatic temperament, that the phlegmatic also, because of their delay, so to speak, in hastening to the defence, should be more open to phthisis than the nervous, is also apparent.

I believe that clinical statistics, if taken, would establish the follow-

ing alleged facts, and that most physicians' impressions will now confirm them, viz., that:

The phlegmo-lymphatic temperament is most common among consumptives, and in it the disease most tends to a chronic form.

The nervo-lymphatic being next in frequency, and also in chronicity.

The phlegmo-sanguineous being much less often attacked, and shorter in course toward death or recovery.

The nervo-sanguineous being by far the least often attacked, and much the shortest in course toward death or recovery.

In considering the relations between mortality and temperaments in phthisis, it appears proper to group them under two classes, viz.: those which exist under circumstances favorable to recovery, and those placed under conditions opposed to it.

Taking the first class, I believe it would be found that the percentage of recoveries is higher in the following order: first, the nervo-sanguineous; second, the phlegmo-sanguineous; third, nervo-lymphatic; and, fourth, the phlegmo-lymphatic.

In the second class—that is, in those placed under circumstances opposed to recovery—the percentage of recoveries would be in an exactly reverse order; in short, that, as individuals, the anæmic and phlegmatic are in the order named most susceptible to phthisis, and in them it most tends to a chronic form, and that the plethoric and nervous are in the order named least susceptible to the disease, and most prone to a rapid recovery or death.

In short, that under circumstances tending to aggravate the disease once established, the mortality is higher and the course more rapid in the nervous and plethoric than in the anæmic and phlegmatic, and under circumstances tending to mitigate the malady, the mortality is higher and the course longer in the anæmic and phlegmatic than in the plethoric and nervous. If these statements be true, they could be accounted for by the excessive irritation locally developed by phthisis in the nervous person, and the increased congestion caused in the sanguineous person, leading to rapid inflammatory changes which would furnish an especially favorable pabulum for bacilli.

Having thus endeavored to suggest a rational basis on which to consider the influence of the temperament upon the course and treatment of phthisis, let us refer to its practical application. Sometimes,

though rarely, the other features in the case mask the temperament, or it is so slightly marked in the individual that it cannot be determined. And often the influence of the disease is such as, temporarily, at least, to cause the sanguineous to resemble the lymphatic, and the phlegmatic the nervous, or *vice versa*. Therefore, often the temperament, for a time at least, is not one of the factors necessary to consider in treatment. In many cases, however, it is important, especially in the matter of climate and exercise, and also in no small degree in drugs, and very particularly in the use of alcohol.

Leaving these general physiological idiosyncrasies of the soil upon which phthisis is established, let us turn for a moment to general pathological diatheses. Under these we find of special importance, as influencing the therapeutics, the rheumatic, and the gouty, and perhaps the bilious, meaning thereby those with a constitutionally torpid hepatic circulation. These points undoubtedly should often influence the choice of climate and of medicine. The tendencies of systems saturated by poisons, especially syphilis and malaria, also need special consideration.

Dismissing now the discussion of what may be termed the general aspects of the country to which the bacillus is a visitor, let us turn to the local conditions of the soil upon which he first implants himself, and where his best energies are usually expended. That is, let us consider, in a general way, the respiratory tract in phthisis, first its antecedent, and then accompanying and consequent conditions.

In looking over the records of chest capacity, conformation, and development, among a number of consumptives, at the onset of the disease, it will be found that though a large proportion of them exhibit deviations from the healthy type, yet there are many in whom there is none of any importance, and not a few who possess an unusually fine physique and full chest expansion, so that here is a factor influencing the decision in using chest expansion as a therapeutic agent, either by altitude or exercise. In viewing this branch of the subject there is, of course, to be considered, not only what are the structural conditions, but how far the chest capacity and expansion have been affected by disease, as by pneumonia or pleurisy, or the expansion by neglect of exercise.

Proceeding to the condition of the respiratory tract, the state of the upper air-passages is of importance. That many of the affections,

catarrhal and otherwise, of these passages predispose the sufferer from them to pulmonary phthisis, appears probable, though in what way they do so is dubious, and it may only be that there is a similar disorder of the mucous membrane lower down. However, clinical observation is convincing of the marked improvement in the pulmonary symptoms often following the successful treatment of irritating or destructive affections of the upper portions of the respiratory tract, and the advisability of always attempting their relief.

The propriety of early attention to any laryngeal affection is especially obvious, by reason of the peculiar susceptibility of laryngeal tissues to become the seat of a tubercle. Both precedent and accompanying bronchial catarrhs, on account of reflex irritation from coughing, also because of the danger of primary or secondary development of tubercle, require treatment. Here, as in the larynx, certain inhalations are of great value.

Accepting these views as correct, let us define pulmonary tuberculosis in outline. It is a disease of the lungs and their appendages, which, if unchecked, results in obstruction of the function and destruction of the tissue of these organs, with consequent general tabes and more or less septicæmia: the invariably exciting cause being a microbe, the bacillus tuberculosis, which, after lodging in the tissues, produces around it a substance called tubercle, which is varied in form and substance by certain local conditions of the tissues and certain diatheses of the individual attacked. That the form of the disease is primarily determined by the manner and degree of resistance or acceptance manifested by the tissue invaded, as well as by its structural peculiarities. That the differences in the manner of resistance can be explained on the same lines as have been temperaments.

When the individual attacked has a naturally vigorous, nervous, and circulatory system, his tissues check the spreading of the parasites by surrounding them with inflammatory exudation and warns the system of its danger by general nervous irritation. The pneumonic or inflammatory form.

When the nervous system is defective and the circulatory vigorous, the resistance is less violent, though effective. The congestive form.

When the circulatory system is imperfect and the lymphatic unusually developed, there is little resistance offered to the ingress of the

microbes, as they are scattered more or less freely by the lymph vessels; and if the nervous system is vigorous, this proceeding results in fever and systemic disturbance. The acute tubercular or infective form.

When both the sanguineous and nervous systems are feeble, the scattering of the bacilli and consequent tuberculosis goes on without much fever or general systemic irritation. The chronic tubercular or infective form.

When a bronchitis, a pneumonia, or a pleurisy precedes the bacillary invasion, the local conditions are modified by them as well as by the diathesis or temperament of the individual attacked.

Fibrosis of the lung may ensue as a change occurring in chronic pneumonia, without previous tuberculosis, the case being one of chronic non-tubercular phthisis, which may again later become tuberculous. Fibrosis more commonly follows tuberculosis, which it destroys. It is never a primary disease. It may be both preceded and succeeded by tuberculosis.

The result of infiltration in all forms of tuberculosis is more or less inflammation around the tubercles.

In the pneumonic form there is marked pneumonic consolidation *en masse*, generally limited to a defined area.

In the congestive form occur the same signs in a modified degree.

In the acute infective variety, the tubercles are widely scattered, causing at first more or less inflammation immediately surrounding the tubercles, with free lung between, and soon setting up bronchitis in the radicles of the bronchial tubes. This eruption of tubercles, which is at first discrete, tends to become confluent in parts in which the immediate signs then resemble those of the pneumonic form; though the evidence of isolated tubercles and the acuteness of the case would demonstrate the form of disease.

In the chronic infective form, the tubercles are less widely scattered, though at first giving evidence of intervening healthy lung tissue; later there is an increased eruption of tubercle, with its accompanying local inflammation in these intervening spaces, which may result in giving the same signs over this area as does pneumonic phthisis; in this form the eruption is generally limited to one lobe, usually the upper, though often occurring simultaneously in both lungs.

Thus we have two main forms of phthisis: first, the pneumonic,

which in its first and second stages is localized in one or two definite areas, accompanied by some fever and irritation, and its modified form, the congestive.

Second, the acute infective form, which always tends to be diffused, and is accompanied by more fever and general irritation and acuteness, and less defined pneumonia; with its modified variety, the chronic infective.

The next stage of the pneumonic form, if arrested, may result, first, in death of the bacilli by the surrounding pneumonia and the subsequent removal of both the microbe and its pneumonic envelope, by absorption; or the pneumonia may be in whole or part converted into fibroid tissue, forming a scar with its accompanying contraction, or the infected area may undergo caseous degeneration, then sphacelus, resulting gradually or rapidly in excavation. This cavity may heal and recovery result with more or less contraction, and perhaps some compensatory emphysema around it.

If not arrested, the pneumonia may extend at first, then undergo caseous degeneration, which breaking down *en masse*, and being without a protecting cavity wall, causes progressive ulceration and increase of bacilli in the surrounding lung tissue. Death ensues from these conditions, giving rise to a general septicæmia, or from secondary tuberculosis filling the lung tissue so completely as to prevent the oxygenation of the blood. Thus besides the progressive pneumonia, and the local death, and its resulting septicæmia, the infective form becomes grafted upon the pneumonic.

The next stage of acute infective tuberculosis, if arrested, may result in each tuberculous mass being encysted by a separate pneumonic envelope, in which the tubercle with its killed bacilli are converted into a fibroid inert mass, or into a cretaceous nodule, which may remain inactive, or which excites minute ulceration around it, and the nodule is expelled by expectoration, the ulcers afterward healing.

If not arrested, the infection may be scattered so freely as to result in death from general obstruction of the oxygenating function of the lungs, at the same time the bacilli are carried to other organs and general tuberculosis ensues; the local pulmonary irritation results in general capillary bronchitis, and if life continues, numerous minute ulcerations, from which more or less septicæmia arises; the cough,

expectoration, and fever adding to the exhaustion and wasting, and death from so-called galloping consumption results.

In chronic infective tuberculosis terminating fatally, several cavities generally result, some of which may run together and for a time a pneumonic wall of defence may be built around, so that the condition of the case resembles the pneumonic variety which results in a cavity; from this may follow all the phenomena of the acute infective form, general tuberculosis, etc., or the slower death from extension of the cavity and general tabes.

Thus in the history of phthisis, cases primarily different in form may secondarily become alike.

Apart from death as the result of one or other of these stages or forms of progress of the disease, it sometimes occurs immediately from one of the concomitant accidents that are liable to arise in the course of phthisis, notably from hemoptysis. In the pneumonic form, hemorrhage is not frequent and seldom dangerous; but later, where a cavity is extending, death often occurs from ulceration opening a big vessel. In the first stage of the infective form, hemorrhage is common, though generally slight; but if the tuberculosis is extensive, the hemorrhage may be fatal. Later, when ulceration is going on, hemorrhages are frequent and more liable to be fatal, though not commonly so. When progressive excavation is occurring, the danger of fatal hemorrhages is very great.

The protective power against cold-catching being generally weakened, perhaps more immediately, owing to changes in the elastic tissue of the bronchi, an intercurrent non-specific pneumonia or bronchitis may block the remaining useful portion of the lung, and so death ensues earlier than the progress of the phthisis would otherwise cause it.

Pleurisy and its results may also precipitate the end.

As also may concurrent or consequent affection of the digestive or urinary tracts, or secondary tuberculosis elsewhere.

A not uncommon cause of death is heart failure from accompanying cardiac disease, especially from dilatation, which, as Pollock has shown, if unchecked, is often the cause of death from hemorrhage.

In extensive fibrosis of the lungs, in which the bacilli are no longer active, death may occur from tabes as the result of oxygen starvation, or from septicæmia caused by the loss of elasticity preventing the clearing out of the dilated bronchi.

CREASOTE AS A REMEDY IN PHTHISIS PULMONALIS.

By BEVERLEY ROBINSON, M.D.,
PROFESSOR OF CLINICAL MEDICINE IN BELLEVUE HOSPITAL MEDICAL COLLEGE, NEW YORK.

THE use of creasote in the treatment of phthisis pulmonalis dates back to 1830, the year in which it was discovered by Reichenbach, of Blausko, in Moravia. Later on, it fell into disuse, like some other valuable medicaments, and for nearly thirty years previous to 1877 it was practically of little or no importance in the therapeutics of pulmonary disease. At the date just referred to, Drs. Bouchard and Gimbert again revived interest in this drug by publishing a very complete article in the *Gazette Hebdomadaire*, of Paris,[1] on its beneficial effects in consumption. They claimed for it excellent results, and for careful, learned, and honest observers their account was as nearly enthusiastic as thorough work is apt to be.

Shortly after reading of the observations of Bouchard and Gimbert, I began to use creasote in the treatment of pulmonary phthisis, and in a clinical lecture delivered at the Bellevue Hospital Medical College, and published in the *Medical Record* of September 21, 1878, page 223, I mention in what manner I have used it and with what results in the following terms:

"For several months, both in the Outdoor Department of the New York Hospital, at Charity Hospital, and elsewhere, I have given dessert-spoonful doses of the mistura creasoti of our *Pharmacopœia* to lessen the quantity of sputa in phthisis. I am very much pleased with this remedy, and believe it merits a very extensive trial. No doubt, in lessening the abundance of expectoration, it also diminishes the frequency of cough."

At that time, I would add, I regarded creasote as being only a good anti-catarrhal agent, to be ranked high amongst some analogous remedial substances whose main action consisted in the diminution of the amount

[1] Pages 486, 504, 522, and 620.

of purulent sputa, but which had little or no curative effect upon the essential lesions of pulmonary phthisis, as we understand them.

From 1878 until 1885, the year when Jaccoud's treatise on pulmonary phthisis, translated by Montague Lubbock, was published, I continued to make use of creasote from time to time, but without watching carefully its effects, or believing that we had discovered, in the employment of this drug, a very valuable addition to our usual medicinal means of treating pulmonary phthisis. Jaccoud's statements about the advantages resulting from the internal exhibition of creasote in this disease impressed me very much, and from the time I first read them to the present date, I have prescribed creasote very frequently in pulmonary phthisis, and, usually, with marked beneficial effects.

About the period when Jaccoud's work was first published, the employment of antiseptic inhalations in the treatment of this disease interested me, and amongst the substances of which I made extensive use none seemed to me of more value than creasote. A great deal of what I could even now say with truth, in regard to the utility and evident results of antiseptic inhalations in the treatment of pulmonary phthisis, can be found by reference to a paper read by me on this subject, at the second annual meeting of the American Climatological Association, held in New York City, May 27, 1885, and, also, in a clinical lecture entitled "Modern Methods of Treatment of Pulmonary Phthisis,"[1] which was delivered before the students of the Bellevue Hospital Medical College, October 27, 1885, and was published in the *New York Medical Journal* of November 14, 1885, page 535. In the issue of the *Journal* of that date, the following occurs:

"I have employed at different times a large number of inhaling fluids and many different combinations. The fluid and combination to which I now give the preference are creasote and alcohol, equal parts, to which I also frequently add a like proportion of spirits of chloroform. This combination is certainly very useful in allaying cough and modifying the quantity and quality of the sputa in pulmonary phthisis. I, therefore, recommend it very warmly. The alcohol is added to the creasote for the double purpose of diluting it and making it more volatile; the spirits of chloroform is added, in view of the experience of Dr. J. Solis-Cohen, of Philadelphia, to diminish local irritation and excessive cough. . . . Properly and judiciously employed, the creasote inhalant relieves symptoms notably, and in the beginning, at least,

[1] This paper was published in the Transactions of the Association. D. Appleton & Co., New York, 1886.

of pulmonary phthisis is, I believe, a means of decided utility, so far as the possible arrest of the disease is concerned. It is important that *beechwood creasote be employed.* . . . Precisely the way in which creasote is most useful is, perhaps, difficult to state. By its antiseptic action it is possibly destructive of bacilli; by its local action and general effect it is certainly of value in combating catarrhal conditions. Where purulent cavities exist, it tends to destroy, or neutralize, putridity. These are certainly sufficiently good reasons for its use without pursuing the inquiry further. At all events, these inhalations do good. The physician notices it, and the patient affirms it. In many instances they allay cough better than any cough mixture, and they are certainly free from the great objection of destroying appetite, as opium and morphine so frequently do."

I mention the preceding facts to show, as I trust I have been able to do, that *my* interest in, and experience of, the good effects of creasote in the treatment of pulmonary phthisis date back already several years, and is by no means the outgrowth of later observations which have come to us mainly from Germany, and at the hands of Fräntzel,[1] Sommerbrodt,[2] Guttmann,[3] Lublinski,[4] etc.

And here I would like to add a word of praise for that very distinguished therapeutician, Dr. Dujardin-Beaumetz, who, in his remarkable work on clinical therapeutics,[5] has given corroborative testimony which, in my mind, has increased the importance of creasote as a valuable therapeutic addition to our ordinary arsenal for the relief of phthisical patients. In the latest edition, also, of the admirable work of R. Douglas Powell,[6] will be found remarks of considerable value with respect to the indications for the employment of this drug. In this author's observation, he has been led strongly to doubt whether creasote can be given during the hectic stage in sufficient quantities to influence pyrexia without running a grave risk of setting up gastro-intestinal irritation and destroying appetite. He regards it as being of more value in apyrexial conditions; he adds, however, that "when there is much local disturbance of the stomach and upper bowel, small doses of creasote in combination with opium are sometimes of great service."

I do not wish further to weary your attention with mere biblio-

[1] Deutsche med Wochenschrift, No 14, 1887. [2] Berliner klin Wochenschrift, No. 15, 1887.
[3] Zeitschrift f. klin. Med., Berlin, 1887, xiii. 488-494; and Boston Medical and Surgical Journal, August 18, 1887, p. 161.
[4] Deutsche med. Wochenschrift, Leipzig, 1887, xiii. 829.
[5] Leçons de Clinique Thérapeutique, t. 2. Paris, 1885, 4th edition, p 549, et seq.
[6] Diseases of the Lungs and Pleura, etc. William Wood & Co, New York, 3d edition, 1886, p. 307.

graphical research, and refer to all the observations which have been made in regard to the creasote treatment of pulmonary phthisis; suffice it to say, that it has been largely praised; that several late observers, notably Fräntzel, Sommerbrodt, and Guttmann, are inclined to regard it as directly curative of pulmonary phthisis, at least in its initial, or first, stage; that others, on the contrary, are less favorably disposed toward it, and frankly confess that they have been disappointed in it; and have discovered no reliable evidence to show that creasote has any marked beneficial action over and beyond what may be obtained from several other anti-bacillary agents.

Naturally, the discovery of the bacillus tuberculosis by Koch, in 1882, made those observers who gave it internally, or who made use of it in inhalations, or, in some rare instances, by hypodermatic or intrapulmonary injections,[1] since that date, endeavor to establish its utility mainly upon its antiseptic properties, and upon its power "to retard the local development of tuberculosis," which is probable if the experiments on animals of Coze and Simon, in 1883, can be relied on.[2] Hippolyte Martin,[3] also, found that creasote failed to destroy the bacillus of tuberculosis, even in the proportion of 1 per 1000. This fact is said by him to be equally true of salicylic acid (solution of 5 per cent.); bromine (1 per 10,000 and 1 per 1000); phenic acid (1 : 1000); *quinine* and corrosive sublimate (1 : 1000). In experiments[4] undertaken by C. T. Williams in 1883, with respect to agents which check the development of bacilli most, it was found that *quinine had manifest action in preventing their development.* These properties and this power are not spoken of, and were not probably thought of, as they now are, by Bouchard and Gimbert. They are of the opinion, however, that creasote acts locally on the pulmonary lesion, and produces marked effects which are also beneficial. Essentially,

[1] See Wien. med. Presse, 1888, xxix. 87; Medical News, June 23, 1888, p. 696; American Journal of the Medical Sciences, February, 1888, p. 179; New Medications, by Beaumetz, p. 192; Lyon Médical, 1885.

[2] According to Schill and Fischer, who mixed tubercular sputa with many different substances and afterward inoculated them in different animals, creasote does not appear to have any inhibitory action on the tubercular virus. (v. Mittheilungen aus dem K. Gesundheitsamte, 1884.)

[3] On transformation of true or infectious tubercle into an inert foreign body under the influence of high temperatures and various reagents. Arch. de Phys., 1881, p 93; Revue de Méd., 1882, t. ii. p. 905, 1883, t. iii. p. 209—quoted by Beaumetz in New Medications, translated by E. P. Hurd, M.D.—foot-note on page 182.

[4] Proceedings of Royal Society, No. 231, 1884, quoted in work on pulmonary consumption, 2d ed., Philadelphia, 1887.

they believe that creasote has the effect of promoting the growth of fibrous tissue around an area of consolidated, or broken-down lung structure. Thus it is, with the passage of time, that the cheesy infiltrations in the lungs become absorbed little by little, as the softened masses (or the contents of large cavities) are expectorated and the surrounding walls close in upon a relative vacuum where previously necrosed tissue in a solid or liquid form was largely present.

As will be seen by my own observations in a few cases later on, I am inclined to share this view and believe in its exactness.[1] In regard to it, Jaccoud writes as follows: "Creasote seems also to have some effect upon the fundamental lesions themselves, and to promote the sclerotic change by means of which recovery is found to occur in this disease." (Loc. cit., p. 156.) Indeed, Jaccoud has remarked frequently, after the bronchitis had disappeared and stethoscopic signs of a pulmonary lesion were reduced to a minimum, that two or three months later there was an evident and secondary diminution in the extent of the affected area, and *bronchial breathing* and *bronchophony* on the periphery of this area. These signs accompanying an evident improvement in the general condition seemed to indicate evidently a sclerosis around the area of pulmonary softening.

As regards the anti-bacillary effects of creasote when taken internally, or by inhalation, or both combined, I have nothing new or very positive to offer. On two occasions, in my own experience, when ordinary care had been taken by a good examiner, bacilli which previously had been present in considerable numbers, subsequent to treatment had notably decreased, or completely disappeared. In other cases the examinations made did not permit me to form a reliable judgment in this regard. As to whether creasote interferes with the bacilli locally, or through the circulation in virtue of its antiseptic properties, or whether, in addition to its promotion of sclerosis, it merely favors general nutrition while acting happily upon secondary, though important symptoms, I am not prepared absolutely to affirm. I would add, however, that I am inclined at present to accept the latter, rather than the former belief. This conviction is based mainly upon what seems to me to be a fair and proper interpretation of

[1] This view would appear to be that, also, of Spencer, who, in speaking of the antiseptic treatment of phthisis, concludes that in our treatment it is our aim to promote healing of the damaged lung tissue by means of *fibroid substitution*. (British Medical Journal, January 28, 1888.)

numerous facts observed by myself and others already referred to. It is important to note, however, that I am now decidedly of the opinion that patients, as a rule, improve more rapidly and surely upon the conjoined treatment by means of antiseptic inhalations and creasote given internally, than they do upon either treatment by itself. I have attained this conviction by carefully watching the effects produced when one or other of these methods was abandoned for a time, and afterward when both were resumed and systematically used. It is probable, therefore, that in many cases the frequent, or prolonged, topical application of creasote vapors to the respiratory tract in a considerable area is of undoubted utility and, after a manner, not *very* different from what I have previously described.

Another fact of great practical importance relates to the purity of the drug and the source from which it is obtained. For the information of those whose attention has not been directed closely to this matter, I would state that in New York City (and I presume elsewhere) much of the creasote which is dispensed is simply crude carbolic acid, obtained from the distillation of coal-tar oil, and commonly called "commercial creasote." It has neither the color, the odor, nor the chemical properties of *wood* creasote, or, what is preferable, of the creasote which is obtained from the distillation of beechwood-tar. I am glad to believe that the ordinary dispenser, in making this pernicious substitution, is himself a victim of ignorance and not of knavery —at least so far as what pertains to the therapeutic use of the drug. And yet in moderate, or large, doses, and particularly with sensitive patients, there is a very great difference in the effects of the two drugs. The one, viz., carbolic acid, may prove distinctly injurious, if not poisonous; whereas the other, viz., wood creasote, when judiciously employed, should be followed by favorable, or perhaps negative results, but rarely, if ever, by manifest bad consequences. I am credibly informed that the only creasote in the market to-day which responds favorably to all, or most, tests of absolute purity is that of T. Morson & Son, an English product, which is mentioned on page 407 of the *United States Dispensatory*, and that of Merck, a German product. Of the two products, Morson's is the one which I prescribe and believe is purest. In order to avoid uncertainty, or risk in prescribing, it is essential at present to designate definitely the creasote that you wish

to employ, and afterward see to it that your prescription is taken to a trustworthy pharmacist.

Through inattention to the foregoing necessary precautions, in two instances reported to me, somewhat poisonous effects were produced by the internal use of carbolic acid, when it was intended that creasote should be taken.

In my judgment, whenever creasote is prescribed, it should be taken, at least at the commencement of treatment, in small or moderate doses. These doses should be continued a long while, or only gradually increased.[1] If an attempt be made, especially at first, to take larger doses of creasote, in the majority of cases stomachal intolerance will soon follow and we shall be obliged either to diminish the amount prescribed, or lessen the frequency of doses, or abandon the treatment altogether for a time. My experience is different from the *personal* experience of a Russian physician, Dr. Bogdanovitch, who found no appreciable benefit from small doses (half a grain four or five times a day), but who, when "he began to take creasote in gradually increasing large doses, beginning with four grains a day, and reaching in about two months a daily dose of forty-four grains, there took place fairly rapidly an unmistakable and permanent improvement in his symptoms."[2] It is, also, different from the reported observations of Sommerbrodt and Guttmann, from the perusal of which Bogdanovitch took his inspiration.

The daily amount of creasote prescribed by me for adults, in private practice, has varied usually from three to six minims, and continued frequently many months without increase, or interruption, or any evidence of intolerance. The ordinary dose of half a minim is repeated every two or three hours. It is given with whiskey and glycerine, according to the following formula,[3] which is that of Jaccoud, the sole difference being that I use whiskey where Jaccoud employs cognac or rum:

℞.—Creasote (beechwood) . . . . . ♏vj.
Glycerine . . . . . ℥j.
Spts. frumenti . . . . . ℥ij.—M.
S.—As directed.

[1] This opinion is corroborated by that of Spencer (British Medical Journal, January 28, 1888), who says that the influence of the antiseptic should be continuous and prolonged.

[2] British Medical Journal, March 10, 1888.

[3] Whenever the mixture is taken according to this formula no addition of water is required, and it reminds one, by its smoky odor and flavor, of slightly sweetened Scotch whiskey.

In hospital practice, for convenience' sake, or rather so as to give the patient a sufficient supply of medicine to last until his next visit to the hospital, I prescribe teaspoonful doses, each teaspoonful containing one minim of creasote, and to it are added two teaspoonfuls of water. This addition is made to prevent irritation of the throat in swallowing the dose. It also obviates irritation of the stomach in some instances. The dose is ordered every three hours, so that if it is taken with absolute regularity, the patient gets eight minims of creasote in twenty-four hours. This is rarely the case, as, owing to sleep or other cause, one or two doses are usually omitted.

The formula which I have used in prescribing creasote is a very good one, as in it the creasote is perfectly dissolved and *sufficiently diluted*, thus preventing it from being unpalatable or irritating. The perfect *solution* of creasote and *its large dilution* are both strongly insisted upon by Bouchard and Gimbert in their original article as being essential points in rendering it acceptable to patients. In Jaccoud's formula, as slightly modified by me, we obtain the well-known beneficial effects of whiskey and glycerine in the treatment of phthisis. I regard it, therefore, as superior to the following, which is the one adopted by Fräntzel:[1]

℞.—Creasote . . . . . ♏xv.
Tinct. gentian. . . . . ♏xj.
Spts. vini rect. . . . . ʒvj.
Vini xeres . . . . . q. s. ut fiant ℥iv.
S.—℥ss ter die ex aquâ.

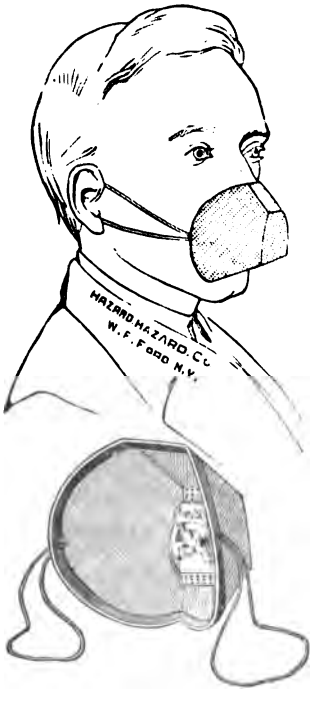
It is well to add that only pure whiskey and glycerine should be employed, and as the latter is frequently adulterated, and hence injurious, I would indicate Price's or Bowers's glycerine as being probably the best.

I have also frequently prescribed creasote in gelatine capsules combined with cod-liver oil. These capsules are now made by several manufacturers abroad, and may be readily reproduced in the United States.[2] Each capsule contains about a minim of creasote. They should be taken fifteen or twenty minutes after meals. At other times they are apt to cause dyspeptic symptoms. Two or three at a dose mark the limit of stomach toleration ordinarily, and in only one

[1] The Year-book of Treatment for 1887.
[2] Already one pharmacist in New York, to my knowledge, makes them.

or two instances have I been able to increase this number without occasioning digestive disturbance. For these reasons the capsules do not appear to me, at present, as eligible a form to prescribe creasote as the mixture of creasote with whiskey and glycerine.

If creasote be administered in cod-liver oil, the amount of oil must be at least one drachm to the minim of creasote, in order to obtain a proper dilution of this drug. Otherwise, if cod-liver oil be indicated for a patient, it is desirable to give it separately, and order the creasote to be taken in the manner first advised. This I have done in several instances, and particularly when, despite the use of creasote, with complete stomachal tolerance, there has been continued loss of flesh.



In a very large proportion of cases of pulmonary phthisis that I have treated during the last year or two (and in every one of the cases hereinafter analyzed), whilst creasote was taken internally, antiseptic inhalations were also used by means of the perforated zinc inhaler

which is here pictured. As a rule, in the beginning, the inhaler was worn during fifteen or twenty minutes every three hours, and from ten to twenty drops of the inhaling flûid were poured on the sponge of the inhaler at least three times in twenty-four hours. The inhaling fluids most frequently employed by me were:

1. A combination of iodoform, creasote, eucalyptus, chloroform, alcohol, and ether, seemingly a somewhat formidable mixture in view of its numerous constituents, but a very rational one when explained in detail.

2. Iodine, creasote, carbolic acid, and alcohol.

3. Creasote and alcohol.

The first one of these inhalations is a modification of one taken by me from Lauder Brunton; the second is that of Dr. Coghill,[1] and, according to R. Douglas Powell,[2] "is a favorite and much used one;" the third is, so far as I know, my own combination.

The following are the precise formula:

℞.—Iodoformi	gr. xxiv.
Creasoti	♏iv.
Ol. eucalypti	♏viij.
Chloroformi	♏xlviij.[3]
Alcoholis ætheris	ää q. s. ʒss.—M.
℞.—Tinct. iodi ætherealis,	
Acidi carbolici	ää ʒij.
Creasoti	ʒj.
Sp. vini rect.	ad. ℥j.—M.
℞.—Creasoti	ʒj.
Alcoholis	ad. ℥ss.—M.

I desire now to direct your attention to the tabulated statement of the results obtained by me from the creasote treatment of pulmonary phthisis.

The total number of cases which have taken creasote mixture and used creasote inhalations (simple or compound) to which I have reference, are: At the New York Hospital, out-patient department, 143;

[1] Antiseptic Inhalations in Pulmonary Affections. By J. St. Clair Coghill, M.D. Brit. Med. Journ., 1881, vol. i. p. 841.

[2] Loc. cit. p. 308.

[3] The chloroform in this formula was originally added by me on account of its value as a preventive of cough. I am glad to find that, according to Salkowski (Deutsche med. Wochenschr., April 19, 1883), it is also *most available*, from its volatility, amongst the ethylic chlorine compounds, as a respiratory antiseptic.

in private practice, 19. Besides, I have an interesting letter from my late house-physician, at St. Luke's Hospital, Dr. Charles H. Collins, who, at my request, looked carefully over the records of that institution and found that during the last two years about 150 cases of phthisis pulmonalis have there been put on the creasote treatment, and a large percentage of these cases he has been able to watch. The points of interest observed by Dr. Collins, and the reflections made by him in his letter to me I will reproduce, after giving an analysis of my own observations—recorded by myself with some care.

Of the total number of 143 cases seen at the New York Hospital, there were 51 cases of pulmonary phthisis at the first stage of the disease; 18 cases at the second stage, 18 cases at the third stage; there were also 4 cases of laryngeal phthisis, 1 case of fibroid phthisis, and 1 case of acute phthisis. The total number of cases, therefore, in which the diagnosis is mentioned of the stage and nature of the disease, is 93—leaving 50 cases of pulmonary phthisis in which the stage of the disease is *not* mentioned. Of the whole number of cases seen at the Out-door Department of the New York Hospital, 54 were females, 89 were males.

Of the 93 cases mentioned, there are 47 cases in which some notes were made as to the effect of treatment, of more or less value. Of these 47 cases, I have arranged in tabular form such data as seemed to me to be of any interest. I have also done a similar work in regard to my 19 private cases. From these tables I shall now proceed to take such facts as result from their study.

The duration of time during which these cases were treated varied from one week to two years eleven and a half months. Of these 66 cases, 45 were males, and 21 females. 37 cases were affected with the first stage in a manifest manner, as shown by the physical signs and the rational symptoms; in 3 cases the physical evidences of disease were doubtful or negative, although the rational symptoms pointed plainly to beginning phthisis; in 6 cases there was found an evident second stage of phthisis; in 1 it was a question whether the case had advanced so far as the second stage; in 11 cases the disease had attained the third stage; in the remaining cases the diagnosis of the stage of disease is not recorded.

In cases of the first stage of the disease, 24 had their cough *improved*, sometimes very much, sometimes only a little; in 3 cases the

cough did *not* improve; in 10 cases the cough was *cured.* In several cases in which the cough was improved, the sleep was quieter, and previous insomnia evidently depended largely upon cough and expectoration; in a few instances, even though the cough improved, the sleeplessness did not improve, and evidently was independent of the cough. In those instances in which the cough is stated as being cured, I cannot say in all of them how long the cure lasted; in some, I know, the cough returned, but was again cured by the use of creasote in mixture, and as an inhalation. In many cases at different stages (first, second, and third)—17 in all—no mention is made of the effect of creasote on the cough. In some of these instances it is possible that the question was *not* asked; in many of them it is probable that *no mention* is made, because the cough remained *stationary.* What I say here for the cough, I should be obliged to repeat for other symptoms, and I would, therefore, offer this as an explanation where my silence shall point to it, without my wearying you by similar repetition.

In 3 cases of phthisis at the second stage, cough improved either slightly or very much. In the other cases it remained stationary; in no case did it increase. In 6 cases of phthisis at the third stage, the cough improved notably in 4; in 1 the improvement was very great; in 1, instead of improving, it became worse.

As regards *night-sweats* at the first stage, 8 cases were cured; 4 improved; 3 remained stationary; in 1 case they increased; in 6 cases the patient never suffered from them; in 15 cases no mention is made of this symptom. At the second stage, 1 case was cured, 1 remained stationary. In a doubtful case of second stage, there was great improvement in one instance. In 4 cases no mention is made. At the third stage, 1 case was cured, 2 improved (1 greatly), 1 patient never had night-sweats, in 7 there was no mention of them.

With respect to dyspnœa at the first stage, 15 cases were improved; 4 cured, 1 case remained stationary, 1 case never had dyspnœa; there is no mention in regard to this symptom in other cases. At the second stage, 1 case was cured, 2 cases improved, 1 case remained stationary; in 2 cases no mention is made. At the third stage, 5 cases improved; in 6 cases no mention is made.

The sputa at the first stage diminished in quantity, and improved notably in appearance in 18 cases; in 5 cases the sputa disapeared;

in 3 cases there was no diminution in amount of sputa; in 1 case the sputa increased in quantity; in 2 cases, in which bacilli had been found in the sputa, later on they could not be found. In 3 cases the bacilli were looked for merely to verify the diagnosis, and they were not looked for later on to see if they had disappeared.

Three times the sputa did not change in appearance or quantity, but were raised more easily. The changes in appearance of the sputa were often quite remarkable, and from green and yellow they became white and frothy; less tenacious, less thick. When the sputa diminished in quantity and were less viscid, cough decreased and sleep often improved. At the second stage, the sputa diminished notably in quantity in 3 cases; in 1 case, although they did not diminish much in quantity, they changed their appearance for the better and became less thick and tenacious. At the third stage, the sputa diminished much in quantity in 4 cases.

The *appetite* was improved in 17 cases at the first stage; it remained stationary in 3 cases; in no case did it notably diminish. It improved in 2 cases at the second stage; in 1 case it diminished. In 4 cases at the third stage, appetite increased. In 3 cases at the second stage, and in 7 at the third stage, there is no mention of the effect on appetite.

In 2 cases dyspepsia was occasioned by creasote; in one of these cases the mixture was continued, and the dyspepsia soon improved; in the other case capsules of cod-liver oil and creasote were given, and had to be abandoned altogether.

In 2 cases nausea and gastralgia were evidently caused by the creasote mixture, which was stopped for a while. In 3 cases the medicine caused constipation, in 1 case the constipation remained the same; in 3 cases the constipation was cured by creasote mixture.

In 2 cases diarrhœa was brought on; in 1 case there was considerable pain in the bowels; in 3 cases there was no effect upon the bowels at all; in 2 cases the bowels became more regular; previously there had been alternate attacks of constipation and diarrhœa.

The effect on weight was very notable in many instances. In 18 cases at the first stage, there was increase of weight, the amount of increase ranging from one-half pound to twenty-five pounds. Two, three, and four pounds increase was quite common. One patient gained three pounds in six weeks' treatment. In 4 cases weight

remained stationary. In 3 cases weight was lost, in 1 of these in moderate amount (about two and one-half pounds), due to an acute attack. Previous to this attack, weight had been stationary. In 2 cases there was a loss of five pounds; in 1 of them four pounds were subsequently regained by three weeks' use of malt and cod-liver oil. At the second stage, in 2 cases there was some loss of weight; in 1 the weight remained stationary; in 3 cases there was no mention of it. At the third stage, there was increase of weight in 2 cases, loss in 1, stationary in 1, no mention in 7.

In a large proportion of cases—46 in all—no mention is made of hæmoptysis. In 11 cases at the first stage, no hæmoptysis occurred during treatment; in 3 of these cases hæmoptysis had occurred previously, small or large in amount. In 4 cases a slight, or very moderate spitting of blood occurred, but in all these cases one or several hemorrhages from the lungs had taken place before the creasote treatment was begun. In 1 case at the second stage, hemorrhage occurred during treatment; but in this case several hemorrhages took place before treatment was instituted. In 5 cases no mention is made of hemorrhage. In 4 cases at the third stage, no hemorrhage occurred either during, or before treatment with creasote. In 7 cases no mention is made of it. It seems probable, therefore, from the foregoing statements, that whilst creasote may not, except to a very limited extent, control pulmonary hemorrhage, it does not promote, or occasion it, and may, therefore, be given with perfect safety to those patients who are liable to these recurrences, and, indeed, during the period they actually take place.

As regards elevation of temperature, no record was made in 41 cases. In the others, as well as could be determined, the following is probably a correct statement: In 7 cases fever was cured under creasote treatment, viz., it disappeared and did not return during the time the patient was under observation. In 9 cases fever was notably lessened. In 1 case of these 9, the fever returned for a time when the patient had an acute exacerbation of the disease, which occurred several times during many months, and did not always appear to be occasioned by imprudence, or cold, but was rather the natural outcome of the disease. In 8 cases, so far as could be observed, no perceptible effect was produced on the fever, and it remained about stationary. In *only* 1 case did the temperature rise whilst the patient was under treatment, and then only to a slight degree.

It is fair to assume that in creasote we have, in the treatment of phthisis, an antithermic agent of no mean value.

In 35 cases there was no mention of the effect of the treatment on the strength of the patients. In 26 cases there was manifest improvement in strength. In 6 of these the strength is spoken of as "returned," or "regained;" in 3, as *greatly* improved; in 17 as notably improved. In 1 case strength remained stationary; in 4 cases *strength* diminished.

Pains in chest were cured 8 times; improved, 13; stationary, 2; none in one instance. In 42 cases no mention is made.

Pains in throat were cured in 6 cases, improved in 7, made worse in 3, remained stationary in 2. In 5 cases patient never suffered from pain in the throat; in 43 cases no mention is made. In 1 case of cure it was attributable to the inhalations. In 3 cases in which the pains in the throat improved, the previous hoarseness diminished, more or less, or disappeared entirely.

In the 3 cases in which the pains in the throat became worse, they were thus caused by the local irritating effects of the mixture. In one instance the voice became weaker and more hoarse.

The pulse is noted as being less frequent and stronger in 6 cases; in 2 as normal; in 2 as showing no apparent change and remaining frequent. In the other instances no mention is made.

Generally speaking, there was no change in the appearance, or amount of urine passed. On only one occasion did it apparently increase considerably in quantity owing to the use of creasote; on another it became clearer, where previously it had contained considerable deposits of urates; in a third instance the urine became more turbid. On many occasions it was tested for albumin; either none was found, or the amount previously existing in the urine remained the same. No casts were observed, in repeated examinations, which could be ascribed to the use of creasote, nor did any pronounced dark discoloration occur, such as may follow the internal use of *coal-tar* creasote. In no instance could I detect the odor of creasote in the urine, and in only one did ordinary tests reveal its presence. This was a case of acute phthisis in a young woman who was taking at the time sixteen minims of creasote daily and who was, also, making frequent use of creasote inhalations.[1]

[1] Case is reported further on.

As regards physical signs, I have only 2 cases at the first stage, to report of complete disappearance of every evidence of morbid condition in the lungs. In two other instances, the signs improved so much that it required the strictest construction not to pronounce them cured. In 10 cases at first, second, and third stages, there was slight or decided improvement in the physical signs revealed by careful examinations of the chest.

This improvement consisted in fewer moist râles heard at the apices, in diminished area of dulness, in diminution of thoracic vibrations, of resonance of the voice, in softened, less prolonged expiratory murmur, which was also of lower pitch. Among the cases which I have reported, there have been, in my opinion, at least four apparent cures, if due consideration be given to the effects produced on both signs and symptoms of pulmonary phthisis. These cases are Nos. 1, 2, 7, and 11.

Dr. Charles F. Collins's report, dated St. Luke's Hospital, May 30, 1888, reads as follows:

"In regard to the creasote treatment in phthisis pulmonalis in hospital cases, I have gone through all the records since the treatment was first begun. The notes in the cases, though accurate, are not complete enough to enable me to make satisfactory tables and to draw positive conclusions concerning special points. Then, also, the previous condition of most of the hospital cases is often very bad in respect to hygienic surroundings, often suffering from want of food and rest, so that after admission to the hospital when improvement takes place it is sometimes difficult to isolate the special value of treatment *per se*. Then, too, there are many cases admitted in the last stages and the condition often without hope, so that any results from treatment are not looked for; to keep the patient comfortable is the only attempt by way of treatment that is available. It is also in hospital cases almost impossible to avoid treating specific symptoms, such as night-sweats, wakefulness, diarrhœa, etc.

"During the last two years about a hundred and fifty cases of phthisis have been put on the creasote treatment, and a large percentage of these I have been able to watch, and the following points may prove of value. It never has been discovered that the drug in any way caused gastric distress or intestinal symptoms. It is pleasant to take, and, in the formula you introduced, patients often ask for it

when leaving and take it for a length of time, and I have never known a patient to dislike the mixture.

"As to urinary and kidney symptoms I would add the following: There has never been any perceptible change in the quantity during the twenty-four hours, and repeated examinations chemically of the urine of patients on creasote have failed to reveal any changes; as far as I can judge, have not known it to cause albumin even though continued for months, and many cases suffering from renal complications when admitted to the hospital show no signs of an increase of their trouble in regard to urine when put on creasote treatment.

"In general, I would like to add that this mode of treatment, the inhalations as well as internal administrations, seems to give better results and be more available than any mode of treatment we have followed out. Many cases leaving the hospital have asked for the prescription, and in cases which I have been able to follow and which have conscientiously carried out treatment, as far as can be judged, there seemed to be a lasting benefit and continued improvement.

"It is a matter of considerable regret that I cannot give you records of a number of special cases, but on account of the conditions above mentioned, truthful records of hospital patients suffering from diseases of this character are always subject to many errors."

Having furnished an analysis of my cases, for any errors in which I crave indulgence, I shall now present an abstract of those histories which seem to me worthy of being recorded.[1]

CASE I.—Patient at beginning of treatment had well-marked signs of consolidation at the left apex. These signs disappeared in a great degree, if not altogether; bacilli were no longer found in the sputa; cough and pain in the chest disappeared. Patient continued to suffer from occasional sweats and general malaise, evidently caused by chronic malaria, and which were invariably cured for a time with Warburg's extract.

CASE II.—Patient, at commencement of treatment, had extensive consolidation at the left apex; later the right apex was somewhat implicated; local signs disappeared almost entirely, and all symptoms relieved or, practically, cured, when patient developed a suspicious condition of epiglottis, viz., swelling, redness, and small ulceration (?) on March 15th, which still remains today, May 1st, although slightly improved as regards appearances and symptoms. During time of treatment patient took for *quite a while* ol. morrhuæ, or the hypophosphites, was abroad one summer two and a half months, also had occasional irritation to the chest walls with iodine, etc.

[1] The last three abstracts were appended after the foregoing was completely written.

May 15. Throat nearly well, under treatment with caps. ol. morrhuæ et creasoti, creasote inhalations, and applications of iodoform and morphine to the larynx; latter apparently most beneficial.

CASE III.—This patient did not show any marked improvement in pains, strength, weight, sputa, etc. On the contrary, the symptoms either remained stationary or *got slightly worse*, except dyspnœa which improved somewhat. Local condition remained stationary.

CASE IV.—This case was certainly one of pulmonary phthisis, first stage at both apices. Besides the physical signs and rational symptoms, the microscopic examination of sputa showed it in revealing the presence of tubercular bacilli. Previous to the use of creasote patient had taken cod-liver oil, maltine, quinine, etc., for a cough which had persisted six months without relief. In this case there was an unquestionable malarial element which was favorably modified by the use of Warburg's extract; still there was no doubt as to the existence of phthisical consolidation at the apices of the lungs to which many of her symptoms were evidently due.

On February 3, 1888, I received a letter from this patient in which the following was contained: "Last autumn and this winter I have had less fever than during previous years. I still have fever sometimes, but in a less degree, and at intervals of three to four weeks. No cough for several months until last week, when I caught cold. It is not excessive and is relieved usually by ten drops of cherry-laurel water. I attribute improvement to your practical treatment and the milk and air of Amityville (here I may say she had enjoyed this milk and air previously for several years and yet no curative result had ensued), and I now enjoy better health than I have during the past six or seven years. This month it is one year since you treated me, and during that time I have gained fourteen pounds in weight."

CASE V.—This patient had suffered from dysphonia and cough for several years; not relieved by continuous use during several months of cod-liver oil and hypophosphites, and a visit to Europe. She had lost flesh and strength. One brother had died from hemorrhage of the lungs. Physical signs were not absolute, but pointed strongly to pulmonary deposit at the apices. In a short time she improved so much as to say she felt almost well. She regained strength, cough diminished, sputa disappeared, and temperature became normal. Patient thinks inhaler did her more good than mixture; believes that one or two loose movements each day are caused by mixture, although appetite is improved and there is no stomachal distress; wears inhaler fifteen to thirty minutes every three to four hours, and adds fifteen drops (creasote and alcohol ʒij–ʒj) to sponge on every occasion.

Physical examination of chest shows no changes.

CASE VI.—In this case patient had dry, hacking cough four years; felt weak, and had night-sweats and fever. During past six weeks had lost eight pounds in weight. At different times had six hemoptyses. On physical examination there were positive signs of pulmonary consolidation at the right apex, consisting in dulness, prolonged, rough, high-pitched expiratory murmur,

increased vocal resonance, and thoracic vibrations. At the left apex the signs were not quite so well marked, but were nevertheless sufficient to make the pulmonary condition in that region very suspicious. Under treatment with creasote, pain in the throat disappeared, appetite improved, weight increased, night-sweats were cured, and the patient became stronger. One moderate hemoptysis occurred after thirteen weeks' continuous treatment by creasote internally and by inhalations. Cough persists after fourteen weeks' treatment, but is much less frequent and intense. The local condition of the lungs has improved, although there still remain evidences of solidification. During the last two months patient has given up his trade, and is living in the country, where he has out-door employment. He says that his appetite is now excellent and he feels almost well. His throat no longer troubles him, and the pains in his chest (due to cough) have disappeared. He has no more fever, his expectoration is reduced to a very small amount daily. At no time has he suffered from dyspnœa. In order to test the relative utility of the mixture and inhalations, I told him about ten days ago to give up using the mixture, and to rely solely on the frequent employment of inhalations. He returned and told me that he found his cough had become notably worse. I then told him to try the mixture alone, and to abandon the use of inhalations for a time. He came back to me and told me that he was sure he coughed more than when he was using both mixture and inhalations. I then told him to resume both. Patient finds when he uses *inhalations alone*, his throat becomes dry and irritable. When he makes use of creasote *internally*, at the same time that he is inhaling, he finds that his throat does not become dry, and that his cough is less frequent and harassing. In this case, as in many others, it seems to be apparent that the combined treatment is more useful than either treatment by itself.

CASE VII.—This patient had suffered from obstinate cough for years in the fall and winter months, cough disappears in summer; had pleurisy in right side seven years previously. One brother has pulmonary hemorrhages. A month before beginning treatment caught fresh cold, and cough became more severe; suffered from dyspnœa; lost appetite and some weight.

Physical examination showed evident consolidatton at right apex. After six months' treatment cough stopped; no sputa; breathing much improved; gained weight. One month ago, through exposure and inhalation of smoke at a fire, got renewed cough and dyspnœa, appetite diminished, and expectoration of white, tenacious sputa returned. Slight improvement in local condition.

CASE VIII.—This patient had been losing strength gradually during nine months subsequent to typhoid fever; had loss of voice, cough, and abundant white and yellow sputa during six months. A cavity was easily made out at right apex, and the laryngeal appearances were typical of phthisis. Under treatment rational symptoms improved in every way; soreness over larynx completely disappeared by frequent use of the inhaler (local application of astringents, iodoform, etc., gave him only temporary relief when frequently

applied), and patient's strength and appearance markedly improved. Subsequently creasote treatment was abandoned, and Bergeon's treatment begun. Under the latter he gained weight rapidly, three and a half pounds in one week, and stated that he felt better (notably stronger) than when under creasote treatment, except for his throat, which again became sore and irritable, and in consequence of this the sputa notably increased. Patient again improved in every way, so far as symptoms were concerned, when he employed inhaler with creasote and gaseous injections at the same time; the latter diminished the number of moist râles in the right lung near, or over, *the seat of the cavity.*

CASE IX.—This patient had a very obstinate cough, and lost considerably in weight; had pains in chest, was hoarse, and expectorated moderately, sputa showed numerous bacilli.

Physical examination showed evident signs of pulmonary consolidation at the right apex. He complained of feeling always tired, had diarrhœa and dyspeptic symptoms, which he attributed to malaria. Under creasote treatment he gained weight, improved in strength, cough diminished, pains in chest disappeared as well as sweats, hoarseness improved. After one and a half months' treatment he went to the mountains of Virginia, and there continued to improve in every way, so that he felt himself almost well again. Later he took fresh cold, and cough became more frequent and intense for a time. Nine months later he informed me (February 26, 1888) that he had been in the mountains of Virginia all the previous summer, and whilst there increased twenty-five pounds in weight and felt much better in every way. During his sojourn in the mountains he used daily gaseous injections after Bergeon's method, and continued use of creasote inhalations, especially at night, but also in day-time. They always relieve his cough almost immediately. "Sometimes he wears inhaler several hours at a time; sometimes nearly all night, even during his sleep." He would not be without it for anything; it relieves his cough and breathing so much, it clears him out so well. Finally, after two months' trial he gave up Bergeon's treatment on account of fistula in ano, and resumed capsules of cod-liver oil and creasote, two after each meal, more than this number gave him dyspepsia. After some weeks he found on account of stomach (dyspepsia) he could not take capsules continuously, but was obliged to take cod-liver oil pure one week, and the following week resume capsules. His stomach again became tolerant. He returned to New York City, October 22, 1888, having normal weight, and did well until one month ago (about January 26th), when he began to lose flesh again. During past month has lost seven pounds, still to-day (February 26, 1888) is stronger than last spring, weighs more, has no fever and no sweats; cough far less troublesome—in fact, only annoys him at night, and is usually readily relieved by use of inhaler; sputa are whiter, less thick and viscid, diminished in quantity; appetite fairly good, and he can do much more work than last spring mentally and bodily. Evidently, therefore, he has improved notably in the majority of his symptoms, although commencing to lose flesh again

during the past few weeks. This loss of flesh patient attributes to pains caused by fistula, other symptoms not being markedly worse.

Examination of chest reveals a considerable area of dulness at right apex in front, in which there are present increased vocal resonance, prolonged, rough expiratory murmur, hyper-resonance of cough; some crackling sounds heard during cough in first right intercostal space (which seems to indicate local softening). Right side of chest expands insufficiently, and the lung is evidently bound to chest walls by old adhesions. Left lung is healthy. I advised patient to leave New York City as soon as possible, and to return to the mountains of Virginia. I also advised him to continue use of inhaler, and to use it many times in twenty-four hours, even though cough should not be very troublesome. I also urged him to continue use of creasote internally, ℥ij at least four to six times in twenty-four hours. Cod-liver oil was, also, prescribed in addition to capsules if his stomach would support it. Except for chest condition, locally, I would find this patient improved in every way; cough better, appearance better, no sweats, increased weight over that of last spring, stronger, sputa changed favorably, and much less in quantity. The chest condition is not obviously improved; on the contrary, it is apparently somewhat worse.

CASE X.—This patient was a thin man of small stature, and had slight frame from birth. Two years ago he began to suffer from cough, moderate expectoration, pain in the chest, and loss of weight (five pounds), night-sweats at times, repeated slight hemoptyses. Took cod-liver oil and hypophosphites and several other drugs for cough, and continued them a long while; they never benefited him nearly as much as the creasote mixture and use of the inhaler; began this mixture under my care three months ago, and took it nearly a month regularly. During that time got rid of cough entirely, appetite improved, sputa came up easily, diminished in quantity, and from being green became yellow, pains in chest and throat disappeared, gained four pounds in weight, night-sweats disappeared, fever cured, and gained notably in strength. During nearly two months patient felt as well as ever. Within a few days has caught cold and again has cough, green sputa, and is constipated. When cough was absent, all signs of bronchitis disappeared. Prolonged high-pitched expiratory murmur with rough tone remained. Dulness over certain areas and increased vocal resonance did not disappear. (*Note:* Sputa under microscope within past few days showed numerous bacilli.)

CASE XI.—When case came under my care patient had suffered from obstinate cough during many months; slight expectoration, no fever, some loss of weight.

Physical examination showed consolidation at the right apex; patient felt very weak; she was put on creasote treatment internally and by inhalation. Six months later cough had disappeared and patient felt well, her weight had increased two pounds (during three months of this period she was at North Elba in the Adirondacks). Physical signs were favorably modified, the ex-

pirations being less prolonged and harsh. In month of September, seven months after treatment was begun, patient caught a cold, with increase of cough and expectoration. This yielded in a few weeks to the creasote treatment. Again patient did very well during a period of six or seven months, during which time she had no cough or expectoration; she then took another cold. One month after the occurrence of this, I saw her again, and found the local condition again somewhat worse. Indeed, there was evidence of some implication of the left lung. Upon close questioning of the patient I discovered she intermitted her medicine when she thought herself well and the cough had disappeared. Eight months later (May 12, 1888) I received a letter from this patient, who during time mentioned had been residing in Nashville, Tenn., from which I take the following: "I am glad to be able to send you a favorable report of my health. During the winter I had only one cold, it was very severe, but I have now gotten completely over its effects. After seeing you last September, I took creasote internally for about two months, and again for one month in February when I caught cold. The only trouble I have now is, that reading aloud sometimes makes me hoarse, especially in the early morning. My appetite is very good, and I can draw long breaths without difficulty, bowels are regular, no fever or sweats, and I have been able to accomplish more work without fatigue than during the two preceding winters. I have not been weighed for some time past, but I know that I have not lost flesh."

CASE XII.—Saw case first time seven years ago. At that time patient had evident tubercular infiltration of the different portions of left lung. Four years previously to date, when I saw her, she had suffered from pleuritis on the left side. At the time first mentioned, and after making trial of cod-liver oil, iodide of iron, etc., internally, antiseptic sprays to throat and counter-irritation to the chest; finding it did not progress favorably, I gave her the creasote mixture of the Pharmacopœia ℥ij every four hours. The effect of this mixture, during the six weeks she took it, was to lessen pain in the throat, to diminish amount of expectoration, to lower temperature, to decrease frequency of cough, and to make patient feel stronger and in better health and spirits, although body-weight remained stationary.

Six years later patient came again under my professional care, mainly because she was suffering from laryngeal phthisis at the stage and in the form of ulcerative disease. Since I had seen her she had lost much flesh and strength, and had suffered for several years from presumably tubercular disease of the spine, which, under judicious treatment, had very greatly improved. Patient's spirits were very good, as he was by nature very buoyant and hopeful. I immediately placed patient on mixture and inhalations of creasote, and made applications to her larynx of iodoform and morphine. After some weeks' treatment patient went to the mountains for the summer, during which time treatment was faithfully continued. Five months later she returned to New York not materially improved so far as lung or throat condition was concerned. The apex of the left lung was the seat of a moderate size phthisical cavity, and the right lung at its apex showed evident consolidation. One or

more of the ulcers of the larynx seen last spring were undoubtedly cicatrized, but new ones had appeared. She complained of dysphagia, of soreness over larynx, and of obstinate cough. Patient remained under my care, in New York, from October, 1887, until December, 1887, when, unimproved, she went to Colorado Springs. During these two months and subsequently she continued creasote treatment most of the time.

On January 27, 1888, Miss M. wrote me a letter, from which I take the following "My throat is much better and seems to be improving all the time, though slowly, of course. Dr. T. said yesterday that the throat looked better than it has done since I have been here. The ulcer is better and there is much less swelling. Dr. T. does not think it necessary to change the treatment—*i. e.*, the creasote mixture and inhalation in any way, as I am doing so well now." Later on, in same letter, she speaks of improved power of deglutition and of her ability to swallow eggs, meat-balls, and bread and butter. I received a letter from Dr. T., dated April 23, 1888, in which he speaks of new ulcer in larynx doing well, and states "no material change in pulmonary lesion," although patient has lost strength. Pain in throat and ear were present, and patient suffered from occasional attacks of dyspnœa, probably cardiac in origin. For the bright side, Dr. T. speaks of the appetite as wonderfully good, and of the patient as being "bright and cheerful always." About six weeks before writing me the creasote mixture (Jaccoud's) had been discontinued on account of dysphagia, and during a few days the inhalations were omitted, but later were resumed. Dr. T. mentions that the pulse, which had ranged a few weeks since as high as 130 occasionally, now keeps steadily about 96, and is of fair strength and volume.

Patient is still at Colorado Springs (Dec. 1888) and has greatly improved in health. I am informed by her brother that latterly she has given up the creasote treatment, but for what reason, he could not tell me.

CASE XIII.—This patient, who had lived on a ranch in Wyoming Territory for several years, had suffered much from exposure, bad food, mental anxiety, etc. He had a cough for several months, and physical examination of chest showed signs of incipient phthisis. He was immediately put on creasote and cod-liver oil, which was subsequently changed to creasote mixture on account of stomach intolerance. Five months later he had improved somewhat in weight, strength, and general appearance. The appetite, cough, and dyspnœa were also somewhat improved, as well as the local signs in the chest. Owing, however, to the lack of fidelity on the part of the patient to the use of the creasote mixture and creasote inhalation, he did not improve as much, I am confident, as he would have done otherwise. During the winter I sent my patient to South America and back for the good effects of an ocean voyage. Apparently he derived no benefit from it.

Patient died of advanced pulmonary phthisis on December 9, 1888.

CASE XIV.—This patient complained of weakness, cough, abundant sputa, hemoptysis, and loss of flesh during several months. Physical examination showed consolidation at the right apex. Immediately put on inhalation of

creasote and alcohol, equal parts, ten drops to be inhaled every three hours. In addition, she had, as internal treatment, at different times, cod-liver oil, the hypophosphites, and arsenic; also, counter-irritation to the chest walls. At the end of a month, patient reported slow fever in evenings, cough *diminished*, sputa more foamy, and less thick and green than they were previously. One hemoptysis (slight). Sleep improved on account of diminished cough; appetite good; strength improved. Less tightness of chest. Slight improvement of local condition of lungs (once or twice during the month, when patient was using the inhaler twenty minutes every three hours, and renewing the drops each time throat was irritable and cough increased, by decreasing time to ten minutes, and pouring drops on sponge of inhaler less frequently, cough again lessened, and was not so distressing). *Note:* At first, during several days, in a certain proportion of cases, cough will be increased by inhaler; later, it will usually diminish or disappear.

Case XV.—This patient came under my care twenty-two months ago. At that time she had a cough, abundant purulent sputa, loss of flesh, slight daily rise of temperature, occasional chilly feelings, and was very weak. She had never spat blood. The right lung contained a quite large excavation at the apex. The left lung was in fairly good condition, showing a certain amount of pleuritic thickening and generalized bronchitis of the larger tubes. This patient, who had tried extensively cod-liver oil, the hypophosphites, and appropriate changes of climate, was placed by me on the mistura creasoti and creasote inhalation, under which treatment many of her symptoms improved. notably during the summer. From that time until April 22, 1888, a period of twenty-one months, I had little or no knowledge of her, when her husband wrote me a letter from Kittrell, N. C., from which I extract the following: "We are keeping up the fight bravely, in Mrs. L.'s case, and I can almost say she is as well as two years ago. These two years have been years of varied treatment with medicine and climate. After leaving New York, in 1886, we continued your treatment (*i. e.*, creasote mixture and inhalation) for some months. I think the creasote internal treatment was of unquestioned value. The stomach was, and continued in better condition for a long period, than it had been for some years. Of the inhalation, I cannot say so much. Its effect, after continuous use, was too drying, irritating to the throat, due, I presume, to the ether. It had to be suspended frequently, but when resumed, produced the same results, and, after a quite severe acute attack it was definitely, under her physician's advice, given up. Since then her treatment has varied much. For several months the Bergeon treatment was used with very questionable results. She remained in St. Louis all the winter of 1886–87 in comparatively good health. Last summer we spent in Dublin, N. H., where we remained four months without calling a physician." "Mrs. L.'s left lung is absolutely sound. The breaking down of the right lung has not apparently increased. Her weight is about the same as two years ago, and strength not perceptibly less. She walks two or three miles on a level without fatigue, though, of course, any up hill produces shortness of breath." Throughout

this whole period there has been a slight evening fever, though ordinarily imperceptible to her, say half a degree to a degree; in the acute attacks rising to two or three degrees. She sleeps better; appetite better; coughs less at night than two years ago, and her stomach and digestion are, on the whole, better, though, of course, subject to occasional slight disturbance.

Early in May Mrs. L. came to New York, and I saw and examined her on two occasions. In reply to my questions she says she is stronger, coughs less, expectorates less, and sputa whiter in appearance, and has a better appetite and digestion than two years ago. Her former night-sweats are cured, and at present she has little or no rise of temperature. Her general appearance is much improved, and to look at her one would hardly suspect grave disease. She has, indeed, gained several pounds in weight during the past three years. The cough at present annoys her very little during the day, and the cough and expectoration are principally limited to night and morning, when she empties her cavity. Physical examination of chest showed that the cavity at the apex of the right lung has not contracted in a very notable manner during the past two years. It seems to be empty, as few or no gurgles are heard over its site. Left lung is apparently in quite good condition relatively, although expiration is prolonged, high-pitched, and harsh under left clavicle. The left lung is doing double duty, as is shown by the extension of resonance and the very complete dilatation of the parenchyma. The area of the left lung, by its inner margin, goes considerably beyond its normal limit toward the right.

On several occasions, latterly, this patient was obliged to interrupt the use of creasote internally, because it apparently caused unpleasant cardiac palpitations after taking it a week or ten days regularly. I ordered her to take one grain strophanthus tablets the next time she experienced palpitations whilst taking creasote internally, and to repeat them four times in twenty-four hours, hoping in this manner to obviate the unpleasant symptom and to be able to continue a useful remedy.

Patient died in New York, under my care, Nov. 3, 1888. Dr. G. L. Peabody made the autopsy in my presence, and found the intra-pulmonary conditions to be essentially those which I had recognized as existing before death.

CASE XVI.—This patient is a lady, nineteen years old, who resided at Farmington, Conn., when she came to see me professionally, April 12, 1887. She had been nursing during several weeks, a brother who died of advanced pulmonary phthisis, at the age of thirty years. During the illness of her brother my patient had endured much fatigue and anxiety. She lost her appetite, a considerable amount of flesh, and began to suffer from an obstinate, dry cough. Examination of her chest revealed the physical signs of pulmonary consolidation at the left apex. Immediately she began the use of creasote mixture and creasote inhalation.

On May 24, 1887, there was little improvement in symptoms. On May 12, 1888, nearly one year later, I received a letter from this lady, who was then in Georgia, who writes: "I came south last September after passing the

summer in Connecticut. I took the creasote mixture and inhalations until I came south. Since then I have been quite well, and gained *in weight* until about a month past. I think it is due to my taking cold and being so very busy. I am thinking of sending home for the creasote mixture. I am very glad you asked me to give a full account of myself, as I wanted you to know that I still expectorate every morning and often during the day."

CASE XVII.—This patient called to see me first on October 27, 1887, in company with his medical adviser, Dr. S. W. Gerow, of New Palts, N. Y. He had a tickling cough lasting eight months, with some improvement during the summer, but never entirely disappearing, had lost some flesh and strength. Expectoration very scanty, no hemoptysis.

Examination of right lung gave all the physical evidences of the first stage of phthisis. Creasote treatment was advised. On May 24, 1888, seven months after beginning this medication, I received a letter from Dr. Gerow, from which I extract the following verbatim: "The patient, S. G. J., did not use inhalation, but commenced at once the use of creasote as advised and with the best of results. Cough gradually improved, appetite and weight increased, and he has again resumed his usual occupation of bookkeeper. Has at present very little cough, some hoarseness. The area of dulness in apex of right lung much less."

CASE XVIII.—This patient came to consult me in regard to his throat at the request of Dr. Henry Schweig, of New York City. He was suffering from dysphonia, dysphagia, some dyspnœa, loss of flesh, slight elevation of temperature, moderate expectoration and cough, and slight night-sweats. On examination I found an advanced œdematous condition of laryngeal phthisis and evidences most distinct of pulmonary consolidation at the right apex, Under creasote treatment internally, this patient improved in a remarkable manner. His cough became far less troublesome, his pain in throat disappeared almost entirely, his power of deglutition improved, his dyspnœa was lessened, his general appearance was improved, and from pale and waxy looking, he became quite healthy in aspect. The sputa did not decrease, nor were they modified favorably in appearance; expectoration was made easier.

Patient said that he felt very well, except for incomplete loss of voice and considerable remaining dyspnœa due to the intra-laryngeal condition, which was still that of pronounced swelling and stenosis due to œdematous infiltration. Examination of chest after one month's treatment revealed much smaller area of dulness at the right apex, less hyper-resonance of the voice, disappearance almost of bronchial breathing. *Larynx* was also improved, and the œdematous condition of the arytenoid cartilages had diminished, and there was less muco-purulent secretion visible locally. (This improved condition of the larynx was presumably due, to a large extent, to the local treatment of Dr. Schweig, which consisted in two or more applications of the galvano-cautery to the infiltrated arytenoid cartilages and usual applications of sprays and powders locally.) I feel constrained to add that the

sputa of this patient, although examined carefully by Dr. Louis Heitzmann, did not reveal the presence of *tubercle bacilli*, but merely the *microbes* (staphylococci and streptococci) of *ordinary inflammation*, which were found in relatively large numbers, pointing to the severity of this condition. This fact has, perhaps, an important bearing on *the correct translation* of some intralaryngeal conditions accompanying pulmonary phthisis, as showing their nontubercular nature, and the possibility of their being greatly and favorably modified by local treatment. This patient died about six months subsequently of laryngeal and pulmonary phthisis.

Appended Cases.

Case XIX.—Lizzie H.,[1] aged thirty-one, United States, school teacher. *Oct.* 26, 1887, patient admitted to hospital. Until April, 1887, enjoyed good health, then began to have cough and expectoration, occasional night-sweats and diarrhœa. In the fall of same year symptoms became more marked and expectoration became muco-purulent, formerly white and frothy. She, during this time, has lost flesh and grown much weaker, being unable to do any work. On admission patient was very feeble, unable to leave her bed. Examination of lungs showed *signs of softening at both apices*. Immediately was put on creasote treatment. Night-sweats at first were frequent, at the end of four weeks became less so, but returned from time to time; expectoration and cough diminished; by slow degrees strength began to return. Appetite, as a rule, good, but frequently for several days there would be a distaste for all food. Patient remained in hospital for five months, and was greatly improved, being up all day, slept well at night, weight increased, and appetite more regular.

On March 5, 1888, patient was able to take the journey to Colorado Springs, with but little fatigue, and improvement there has continued, the same treatment being kept up. In addition to the above it may be said that various and distressing symptoms were treated from time to time symptomatically.

Case XX.—Victor D.,[2] male, aged forty years, single, Belgium, teacher. Admitted to hospital December 8, 1887. Previous history as to time somewhat indefinite; probably less than one year. During last six months has lost considerable flesh and strength, some cough, worse last few weeks, with night-sweats and dyspnœa. Changes at both apices; expiration prolonged, raised in pitch; increased voice and breath sounds; few moist sounds and some pleuritic râles.

14*th*. Patient was put on creasote treatment internally and inhalations. In addition to the above, for almost two weeks, patient was put on potass. iodid. and hydrarg. biniodid.

January 12, 1888. Having been on creasote treatment nearly one month, patient left hospital: cough, night-sweats, and dyspnœa were much better, and treatment was continued at home.

[1] Case reported by Dr. Charles F. Collins.

[2] Case reported by Dr. Charles F. Collins.

May 4, 1888. Patient came to the hospital and reported himself as being in better health than for some time past. He takes a long walk every day, followed by no fatigue; appetite and digestion are good; there is no cough, expectoration, or night-sweats, and weight has been increasing.

CASE XXI.—Acute pneumonic phthisis in a young woman twenty years of age, following first child-birth. In this patient breaking down of lung tissue progressed very rapidly, so that at the end of one month a cavity had formed at the right apex and another one in the lower lobe of the left lung. There was much cough, sweats, purulent expectoration, rapid loss of flesh and strength, and continuous high temperatures. Patient was put on combined creasote treatment, and improved for two weeks in a very manifest manner. Her cough diminished, expectoration diminished, and temperature was lowered. Her strength also augmented considerably; she then took a fresh cold and her improvement ceased for a time. On several occasions her sputa were examined, and each time a considerable number of bacilli were found. As to whether they diminished in number under treatment could not be satisfactorily determined.

The points of interest in this case were mainly two: First, patient took increasing doses of creasote, until she took sixteen minims daily, divided in three doses, after each meal, without showing during several days, even when the latter doses were reached, any stomachal intolerance. Finally, however, this intolerance was manifested, and, on this account, for some days the treatment was stopped, and then resumed again later, with smaller doses several times daily. On questioning this patient, it was noted that she always had very good digestion, and that in her immediate family no one had ever suffered from dyspepsia. The urine of the patient was examined on many occasions, before and after she had taken creasote internally. Before all treatment she had a trace of albumin in her urine. This trace remained the same; it did not increase in quantity, nor did it disappear entirely. When she was taking sixteen minims of creasote daily, internally, and also making frequent use of creasote inhalations, creasote was shown to be present in her urine by a very *marked deep brown coloration*, with the addition to the urine of a few drops of the tincture of the chloride of iron. The iron test was employed at the same time with normal urine, and gave no such reaction, and also with the urine from a patient who was taking salol. In the latter case the iron tincture gave a precipitate of a bluish or violet color.

In this place, and before giving my conclusions to this paper, permit me to add a few words which I believe are important to bear in mind. All of us know how readily one may be deceived by tabular statements, or, indeed, occasionally by reported cases. It is so easy to prove too much! Whilst error, however, often arises from the over-valuation of a particular drug, it is possible to under-estimate the utility of a real addition to our curative means in this line, when

judged after a similar manner. Therefore it is that final remarks or reflections, more or less in the form of conclusions, must frequently be added, so that a correct opinion should be formed of what a writer really believes.

I am convinced, in view of what I have seen, the proofs of which I have stated, and notwithstanding their imperfect character in many particulars, that we have in beechwood creasote a remedy of great value in the treatment of pulmonary phthisis, particularly during the first stage. Not only does it lessen or cure cough, diminish, favorably change, and occasionally stop sputa, and relieve dyspnœa in very many instances; it also often increases appetite, promotes nutrition, and arrests night-sweats. It does *not* occasion hemoptysis, and rarely causes disturbance of the stomach or bowels, except in cases in which it is given in *too large* doses.

There is a fair amount of evidence to show that by its long-continued judicious use, it may and will modify favorably the local changes in pulmonary phthisis, and how it does this I have pointed out previously, as far as I was able. Whether or not it has any direct anti-bacillary effect when given internally, or by inhalation, or both combined (the latter method being, in my judgment, the most efficient one), remains as yet to be determined in a more accurate manner. It is certainly an unobjectionable medicament from any point of view. It is easy of administration, it is adapted to the majority of sufferers from pulmonary phthisis everywhere; it may be used with some advantages at all stages of this disease, even the most advanced, and *in my experience* it has proven itself superior to any other medicinal treatment with which I am familiar.

That in all cases the nutrition is the cardinal factor to be always kept in view in the treatment of pulmonary phthisis, no matter what method or course be followed, is, I believe, as true to-day as it always has been from the clinician's standpoint, and without regard to the passing theories which may be adopted in regard to the precise *rôle* or influence of microbes in the pulmonary structures. The words of Dujardin-Beaumetz[1] seem, in this connection, of much value:

"There do not exist several medications of phthisis; there is but one, that which addresses itself to the nutrition; the others are only adjunct methods,

[1] Leçons de Clinique Thérapeutique, t. 2, p. 647.

which become dangerous if they succeed in affecting unfavorably a single day, a single instant, the digestive functions."

Or those other words of E. L. Trudeau:[1]

"It should be kept in view that so long as the tissues present a favorable nidus for the development of the bacilli, the destruction of a portion of them, if this should be found feasible, would not necessarily eradicate the disease."

To the end of altering those chemical and vital changes in the organism which allow of the growth of the microbe, "thus far those conditions which promote bodily vigor have alone been found effectual."

NOTE.—Since writing the foregoing paper, I am much gratified to find that so distinguished an observer as Dr. Austin Flint has employed the *combined* creasote treatment, internally and by inhalation, with great benefit in ten cases of phthisis pulmonalis (*vide New York Medical Journal*, Dec. 8, 1888).

[1] Medical News, p. 490, May 5, 1888.

THE CONTINUED FEVERS OF THE MISSISSIPPI VALLEY.

By P. GERVAIS ROBINSON, M.D.,
of St. Louis, Mo.

There have prevailed in this region, during the last twelve or fifteen years, very many cases of continued fever, occurring at all seasons of the year and apparently uninfluenced either in frequency or gravity by thermometric or barometric conditions. There is one class of cases beginning distinctly as malarial, so declared by the characteristic phenomena of intermittent or remittent, which either, through the potency of the morbific agent or through coöperation with a second infection (supposably typhoid), resist the ordinary remedies usually effective to combat the malarial poison, which lose, gradually as they progress, their distinctive feature of periodicity and merge finally into the continued type, and to which we give the name of typho-malarial. There is another class of cases whose onset is most insidious, preceded by a shorter or longer prodromic period, soon recognized not only by their continuity but by their characteristic thermometry and by the accompanying phenomena developed *pari passu* with the progress of the disease and the gradual elevation of temperature, and which we designate as true typhoid. Between these two classes of cases we have this intermediate type presenting so great a diversity of phenomena and so little uniformity of symptoms as to create much confusion and to elicit a variety of conflicting opinions as to the pathological cause, and precluding anything like accord as to its classification and nomenclature; and the treatment is reduced to experimentation or to the administration of a variety of remedies totally diverse in their known effects upon the system.

It is with the view of inviting discussion rather than with the hope of shedding any material light upon the nature of so interesting a form of fever, that I propose to give a brief description of the clinical

features drawn from the many cases which have fallen under my individual observation, chiefly in private practice, during the last twelve or fifteen years.

That the diagnosis is obscure is very generally admitted and most practitioners are content to say that they have to deal with typho-malarial fever and claim that greater nicety of differentiation is of little practical importance in a therapeutical point of view. In previous years, and with less experience than I possess to-day, I readily accepted this nomenclature, in at least the majority of such cases, and I was ready to admit the actual existence of such a form of fever as typho-malarial, or rather if not a real amalgamation of the two morbific causes, at any rate, their combined and simultaneous action upon the system; but greater familiarity with the clinical features, and more frequent and repeated observations of the effects of remedies have compelled me to doubt the correctness of this diagnosis, and have forced me to the consideration of the question as to the identity of this type of fever.

The utter failure of all the so-called antiperiodics and antimalarial remedies to control or even to modify the febrile action would seem to exclude malaria, in the ordinary acceptation of that term, as a pathological factor—quinine, the most potent of all these agents, has been given in doses large and small, without other appreciable effects than more or less disturbance of the nervous system and digestive organs; so that to-day its use is very generally abandoned after a few tentative doses, for the first two or three days at most, during the commencement of the attack.

The invasion of the disease is, in a certain proportion of cases, gradual and insidious, as in ordinary typhoid, the patient complaining for some days of general malaise, debility and lassitude, with anorexia, before being compelled to take to bed—in the larger portion a chill is the initial symptom, although seldom so marked or prolonged as in ordinary intermittent fever; subjective sensations of chilliness, however, recur from time to time throughout the duration of the illness, without regularity or notable periodicity, such as belongs to fever of the malarial type.

Symptoms referable to the nervous system are only such as indicate debility and a vague discomfort which can hardly be described by the patient; there is no actual complaint of pain unless in a very few exceptional cases, and in these the suffering is referred to the joints,

without swelling or other signs of inflammation, so that there is a great similitude between them and the dengue or so-called "break-bone" of the Southern States. Now and then insomnia becomes a feature, but seldom leading to or followed by any disturbance of the intellectual faculties, so that delirium of any kind can hardly be said to constitute one of the characteristics of this fever.

Disturbance of the digestive organs, mainly the stomach, in the form of nausea, sometimes persisting for days and accompanied by vomiting of bile and mucus, occurs not unfrequently. There is often complaint of "heart-burn" and a little tenderness on pressure in the epigastrium. No palpable enlargement of the liver or spleen occurs. Diarrhœa is an exception to the rule, the bowels continuing to act normally throughout the duration of the disease, with perhaps a tendency to constipation, and requiring the administration of an occasional mild cathartic. So few are the exceptions to this rule that I can easily recall, from a large number, those rare cases in which distention (tympanitic) of the abdomen or diarrhœa occurred. In such we have to fear intestinal hemorrhage, which, though rare, is, when occurring most commonly, profuse and immediately threatening to the life of the patient.

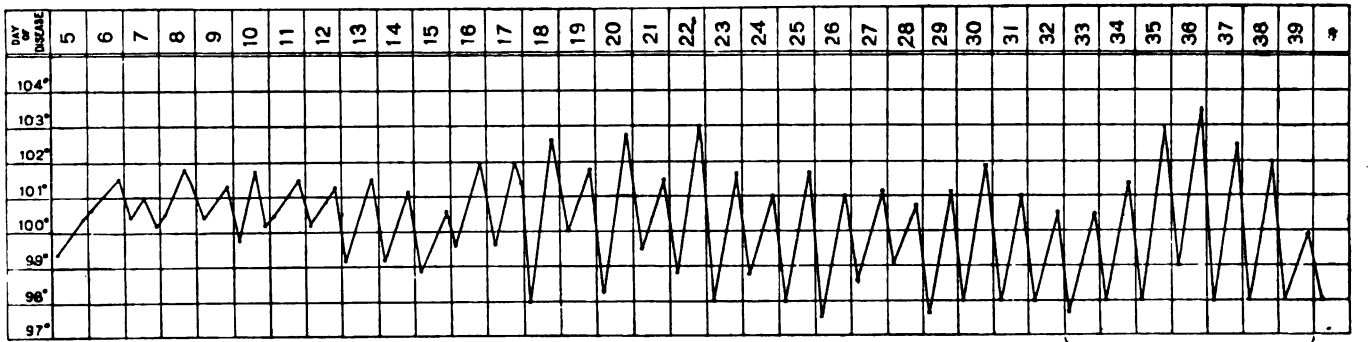
The appearance of the tongue is neither uniform nor characteristic, generally remaining moist throughout the attack, and but slightly coated with a white or yellowish fur, and never becoming dry, glazed, or blackened as in ordinary enteric fever. Nor do we ever observe sordes upon the teeth or gums.

The skin is, for the most part, dry, but is not unfrequently soft and moist, even while the temperature is highest. The pulse is usually soft and more or less accelerated, from 90 to 120, but varies much in rate from day to day, and even hour to hour, and shows no constant relation to the existing temperature, and is rather an indication of the amount of debility than of the fever.

The most prominent feature of the disease is the temperature as shown by the thermometer, which is so remarkable for its irregularity as to constitute a phenomenon almost diagnostic of this form of fever. The accompanying tables kept during their progress will show the behavior of the temperature, in the one case as uninfluenced by any medication, and in the other as somewhat affected by antipyrin.

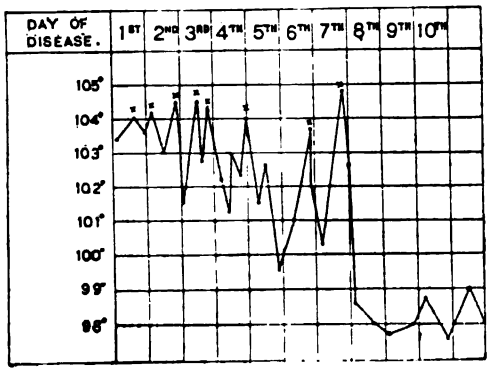
In Case I. the thermometric observations were begun on the fifth

CASE I.



Patient convalescent apparently; up and about the room; appetite and strength returning.

CASE II.



* Antipyrin administered with an elevation of 104⅕° F. The pulse reached 143, and the respiration 50; and there was delirium and a tendency to stupor on the fourth, fifth, and sixth days.

day of the attack, counting from the time when the patient first began to feel unwell. There had been no distinct chill nor rigor, and nothing more complained of than general lassitude and debility. There occurred at no time either headache or pain in any other region of the body. The abdominal walls were relaxed and soft, and the bowels moved naturally every day or two without more than an occasional and minute dose of calomel. There being no special indication for medication beyond the debility and emaciation, and the temperature not being excessive, nothing more was administered than moderate quantities of nourishment and stimulants.

Thus was afforded an excellent opportunity of observing the natural course of this form of fever which would seem to be characterized by one clinical feature alone, namely, the great irregularity of the temperature, which is especially notable during the first few days of the invasion or we might say the forming period of the fever. We note at once how different are the temperature tracings from those which occur in fever of the malarial type on the one hand, and those of typhoid fever on the other.

As happened in Case II. here recorded, the onset of the fever is sometimes sudden, so that a high temperature is reached on the first day, perhaps the highest at any time during the illness, but unlike malarial fever there is no sufficient fall to constitute either an intermission or even a distinct remission. More commonly the elevation is gradual, as occurred in the first case, approximating the curves of typhoid, but not with that regular gradation where the evening and morning temperatures respectively are higher than the day before. When the fever has been fully developed, the course of the temperatures resemble very closely that which marks the progress of phthisis pulmonalis, so that, if seeing the patient for the first time, there should be more or less cough with some physical signs superadded, as slight râles in the upper region of the chest, we might fear we had to do with this disease in its earlier stage. I have seen this error of diagnosis committed on several occasions, which was corrected by the progress and favorable termination of the case.

Defervescence does not always correspond with improvement in the general condition of the patient, since it often happens that convalescence is practically established while there yet occurs a daily rise in the temperature, either in the morning or evening. In some this

defervescence is gradual (by lysis) as in typhoid fever, in others suddenly (by crisis), in these latter the temperature commonly falling below the normal for several days in succession.

The disease is characterized by no constant or uniform indications of any anatomical lesions, while the mortality is so low as to furnish few opportunities for necroscopic investigations.

I am inclined, then, to the belief, founded upon the clinical features and upon the effects of remedies, that we have to deal with a distinct type of continued fever not hitherto described, so far as I know. The classification and nomenclature must be determined by future investigation to discover and mark the identity of the special germ to which most probably its origin is due. The differentiation from malarial fever on the one hand and typhoid on the other is of much importance, since we may thus avoid an unnecessary and even harmful medication, and by expectant treatment permit that favorable termination which nature is competent to effect.

INDEX.

ALBUMIN, in diagnosis and prognosis of kidney disease, 139, 162
Albuminuria, relation of, to life assurance, 171
Angina pectoris and chronic interstitial nephritis, 207
Antiseptic medication, 339

BAUMGARTEN, disturbances of heart rhythm, 232, 243
pulse-wave velocity and ventricular close-time, 262
Bowditch, trophic lesions, 287
Bright's disease, cardiac changes in chronic, 180
Bruen, disturbances of heart rhythm, 242

CARDIAC changes in chronic Bright's disease, 180
Casts, value of, in diagnosis and prognosis of kidney disease, 147, 166
Causal therapeutics in infectious diseases, 109
Chew, chronic interstitial nephritis and angina pectoris, 207, 215
strophanthus in valvular heart disease, 231
Creasote in pulmonary phthisis, 365

DA COSTA, treatment of valvular diseases of the heart, 216, 231
Dana, causal therapeutics in infectious diseases, 120
Delafield, cardiac changes in chronic Bright's disease, 204
paralysis after typhoid fever, 88
Donaldson, diagnostic significance of mitral presystolic murmur, 122, 138
Draper, disturbances of heart rhythm, 243
relations of scientific to practical medicine, 1

EDES, albumin, casts, and renal inadequacy in kidney disease, 139
disturbances of heart rhythm, 242
effect of spinal concussion on the reflexes, 295

FERRIER, trophic lesions, 287
Fevers of Mississippi Valley, the continued, 395
Forchheimer, fatty overgrowth of heart, 89, 107

HEART changes in chronic Bright's disease, 180
fatty overgrowth of, 89

Heart, pin imbedded in, 262
rhythm, disturbances of, 232
treatment of valvular diseases of, 216
Henry, antiseptic medication, 339
Horsley, trophic lesions, 289
Hutchinson, management of convalescence in typhoid fever, 47, 76

INADEQUACY, renal, in diagnosis and prognosis of kidney disease, 151, 169
Infectious diseases, causal therapeutics in, 109

JACOBI, albuminuria, 179
causal therapeutics in infectious diseases, 120
contributions to the anatomy and pathology of the thymus gland, 297
fatty overgrowth of heart, 106
Janeway, value of albumin, casts, and renal inadequacy in diagnosis and prognosis of kidney disease, 163
Johnston, causal therapeutics in infectious diseases, 121
geographical distribution of typhoid fever in United States, 9
relations of typhoid to malarial fevers, 9

KIDNEY disease, albumin, casts, and renal inadequacy in diagnosis and prognosis of, 139, 163
Kinnicutt, convalescence in typhoid fever, 75
fatty overgrowth of heart, 106, 107
temporary albuminuria, 179

LIFE insurance, relation of albuminuria to, 171
Loomis, cardiac changes in chronic Bright's disease, 180, 205
Lyman, disturbances of heart rhythm, 242

MALARIAL fever, its relations to typhoid fever, 9
Medication, antiseptic, 339
Medicine, relations of scientific, to practical, 1
Miles, effect of spinal concussion on the reflexes, 291, 296
Minot, paralysis after typhoid fever, 86
Mississippi Valley, the continued fevers of, 395
Mitral presystolic murmur, diagnostic significance of, 122

NEPHRITIS, chronic interstitial, and angina pectoris, 207
Nervous system, relation between trophic lesions and diseases of, 266, 275

ORD, convalescence in typhoid fever, 74, 76
paralysis after typhoid fever, 86
trophic lesions, 285
Osler, cardiac changes in chronic Bright's disease, 205
disturbances of heart rhythm, 242

Osler, effect of spinal concussion on the reflexes, 295
mitral presystolic murmur, 138
paralysis after typhoid fever, 87
pulsating pleurisy, 330
typho-malarial fever, 45

PARALYSIS after typhoid fever, 87
Peabody, cardiac changes in chronic Bright's disease, 205
causal therapeutics in infectious diseases, 120
chronic interstitial nephritis and angina pectoris, 214
convalescence in typhoid fever, 74
pin imbedded in the heart, 263
Phthisis, causal therapeutics in, 115
pulmonalis, basis for its therapeutics, 354
creasote in, 365
Pin imbedded in a woman's heart, 262
Pleurisy, pulsating, 330
Porcher, albuminuria in yellow fever, 179
typhoid and malarial fevers, 45
Pulse-wave velocity and ventricular close-time, 244
Pye-Smith, functional albuminuria, 179
trophic lesions, 287

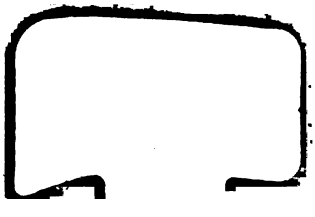
REEVES, sequelæ of typhoid fever, 74
typhoid and malarial fevers, 45
Reflexes, effect of spinal concussion on the, 191
Robinson, B., creasote in pulmonary phthisis, 365
Robinson, P. G., continued fevers of Mississippi Valley, 395
Ross, paralysis after typhoid fever, 77, 88

SANSOM, disturbances of heart rhythm, 243
Seguin, relation between trophic lesions and diseases of nervous system, 266, 290
Shattuck, convalescence in typhoid fever, 76
fatty overgrowth of heart, 106
strophanthus in valvular heart disease, 231
Solly, phthisis pulmonalis, 354
Spinal concussion, effect of, on reflexes, 291
Starr, effect of spinal concussion on the reflexes, 296
paralysis after typhoid fever, 87
trophic lesions, 287
Sternberg, causal therapeutics in infectious diseases, 119
etiology of yellow fever, 321
typho-malarial fever, 44

THACHER, diagnostic significance of mitral presystolic murmur, 137
pulse-wave velocity and ventricular close-time, 244

Thymus gland, contributions to the anatomy and pathology of, 297
Trophic lesions and diseases of nervous system, relation between, 266, 275
Typhoid fever, causal therapeutics in, 109
geographical distribution of, in the United States, 9
its relations to malarial, 9
management of convalescence in, 47
paralysis after, 77
Tyson, fatty overgrowth of heart, 107
relation of albuminuria to life insurance, 171

VALVULAR diseases of heart, treatment of, 216

WILSON, causal therapeutics in infectious diseases, 109, 121
typhoid fever, convalescence in, 76
regional prevalence of, 45
Wood, effect of spinal concussion on the reflexes, 295
fatty overgrowth of heart, 167
paralysis after typhoid fever, 87
relation between trophic lesions and diseases of nervous system, 275

YELLOW fever, recent researches on etiology of, 321